Sidney F. Harmer.

NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

ROYAL INSTITUTION OF GREAT BRITAIN

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS

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WEEKLY EVENING MEETING,

Friday, January 23, 1920.

General E. H. Hills, C.M.G. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

The Hon. Sir Charles Parsons, K.C.B. Sc.D. LL.D. F.R.S. M.R.I.

Researches at High Pressures and Temperatures.

The subject to which I wish to direct our attention this evening is Researches at High Temperatures and Pressures.

Just ten years ago, in this room, Sir Richard Threlfall discussed the effects of temperature and pressure on various substances, and commenced by referring to a suggestion I made in 1904 to sink a bore hole 12 miles deep in the earth with the object of exploring the region beneath us, about which so little is known. Last summer, at Bournemouth, I ventured again to direct attention to the desirability of such an exploration in the interests of science generally, and to the possibility that it might ultimately lead to some developments of practical importance and utility.

Ten years ago no experiments had been made on the behaviour of rocks under the conditions existing at great depths below the surface of the ground; but prompted by my suggestion in 1904, and some subsequent correspondence in regard to the possibility of the rock crushing in and closing the shaft, Professor Frank D. Adams, of McGill University, Montreal, commenced experiments on the strength of rocks to resist the closing up of cavities under the conditions prevailing at great depths below the surface. He published the account of these experiments in 1912, in the Journal of Geology for February of that year.

Adams' method was to place a block of granite or limestone in a tightly fitting cylinder of nickel steel which was shrunk lightly around the block to ensure perfect fitting and support: hard steel rams actuated by a hydraulic press were arranged to press against the ends of the block with a known pressure. Two small holes were previously drilled in the specimen, one axial in the centre and one transverse, the diameter of the holes being 0·05 inch, or one-tenth the diameter of the specimen. The temperature of the container and specimen were maintained at any desired point up to the softening point of steel. In some experiments no heat was applied, in others

the temperature was raised to that estimated to exist at the depth below the surface of the earth corresponding to this pressure.

When no heat was applied the holes in the granite showed no alteration under a pressure equivalent to 30 miles deep, and in the case of limestone the specimen supported one-half of this pressure without alteration. He then raised the temperature of the container and specimen. When granite was heated to 550 C., a temperature corresponding to 11 miles below the surface, it stood a pressure equivalent to 15 miles, and might have stood more but that the container became weakened by the heat. Limestone begins to decompose at a temperature of 450 C., but even at this temperature it withstood a pressure corresponding to 10 miles.

Adams concludes that small cavities in granite will not close in under the conditions of pressure and temperature at 11 miles below the surface, however long a time is allowed to lapse, and that the cavities may persist to much greater depths; but the softening of the steel of the container precluded the carrying of his experiments to still higher temperatures and pressures.

As far as they go these experiments are reassuring as to the permanence and safety of a pit shaft 12 miles deep sunk through granite. But it would be more satisfactory to experiment on a larger specimen than one only $\frac{1}{2}$ inch in diameter, as used by Adams, and to electrically heat the specimen when submerged in graphite while keeping the container cold, the temperature being indicated by a thermo-couple in the specimen; this could be carried out in a nickel steel container like Fig. 5.

In this connection P. W. Bridgeman in 1911 submerged a sealed glass tube containing a cavity under an external hydrostatic pressure of 24,000 atmospheres (corresponding to a depth in the earth of 56 miles) for three hours, and the cavity showed no change in size or form. It, however, appears that temperature will probably place a limit to the depth that would be reached before closing in of the shaft commences to occur, for Judd, Milne and Mallet agree in the view that the deepest origin of earthquakes is between 30 and 50 miles. This would seem to indicate that at greater depths than 30 miles the temperature and pressure are such that changes of form take place by plastic deformation, and not by sudden slips or the formation of faults, which are the chief cause of earthquakes. Again, Oldham states that beyond 20 miles deep seismic waves which are transmitted by compression and distortional vibrations change in character in this respect: that though the compressional waves are only slightly affected in velocity, on the other hand the distortional waves are reduced to one-half their velocity. This would seem to imply that the modulus of elasticity in shear has, at 20 miles depth, fallen to one-half owing to the rise of temperature, and it seems probable that the rock also is weakening in its resistance to shear: in

fact that the rock is becoming more plastic, and that cavities would probably close up at 20 miles below the surface.

The deepest single stage shaft on the Rand is Hercules E.R. P.M. It is 4500 feet vertically and is rectangular in section.

The deepest shaft in the world is Morre Velho. The bottom is 6400 feet vertically below the surface, and it has been sunk, and is worked in stages, two of which are about 1200 feet vertical. The deepest shaft designed on the Rand is by the City Deep Company. It is 7000 feet vertically, is circular of 20 feet diameter, and is to be worked in two stages of 3500 feet each.

The most rapid sinking record was made at the Crown Mines No. 15 Shaft, where 310 feet were sunk in a month. It is circular and of 20 feet in diameter. There are several interesting departures from ordinary mining practice necessary even at this depth. The haulage is arranged in stages of about half a mile, principally in order to economise the weight of rope, and also the power for winding. In countries where the atmosphere is dry the sides of the shaft are cooled by sprinkling water upon them, which by evaporation cools the rock. It is however possible to augment this effect by artificially drying and cooling the air before passing it down the mine.

When still greater depths of shaft are in contemplation, further methods of cooling in addition to these would probably be found necessary—for instance, the carrying the heat upwards by means of brine circulated in a closed ring formed of steel pipes with a rising and descending column. Though the columns might be carried the whole depth of 12 miles, the hydraulic pressure at the bottom would be about 12 tons per square inch, and would entail very costly pipes of great strength to resist the pressure. A cheaper plan would be to work in stages, each ring occupying a stage of from 2 to 3 miles of the shaft, the heat being transferred from the top of one brine ring to the bottom of the ring above by surface heat exchangers and refrigerating machinery to neutralise the heat drop on transfer: these may be called heat pumps and would be driven electrically.

As the depth of the shaft increases the pressure of the air upon the miners will be about doubled for every 2 to 3 miles, but what is more serious is the increase of temperature of the air itself caused by the adiabatic compression due to gravity, by which it will be raised about 100° F. For these reasons it will be necessary to place air-tight partitions across the shaft at every mile or two, and to carry on the ventilation through these by means of a pump to deliver the foul air upwards, and an expander to allow the fresh air to descend; these two machines would be linked together and the difference in their power supplied by an electric motor (this method has been often used with water and is equally applicable to air). At each partition heat exchangers and refrigerating machinery similar to that used for the brine would be placed.

When sinking the deeper portions of the shaft probably shields would be necessary to protect the miners from the splinting of the rock which is caused by the intense compressive stress which splits off scales from the surface, sometimes with considerable violence.

In 1904 the estimate of the time required to sink 12 miles was eighty years and was based on the records at that time. With improved machinery and methods the records have been so much lowered (at the Crown mines 310 feet of a circular 20 feet die-shaft have been sunk in a month) that an estimate of thirty years seems now to be reasonable.

Threlfall traced the gradual evolution of the theory of the effects of temperature and pressure on the allotropic forms of various substances, their critical temperatures and conditions of gaseous, liquid and fluid states. He described his apparatus and experiments designed to melt graphite under high pressure, his inference being, that under pressures up to 100 tons per square inch carbon does not follow the same law as many other substances, and does not crystallize as diamond on cooling.

An interesting discovery was made by Bridgeman in 1911 when studying the compressibility of mercury. He found that it had a remarkable power of penetrating steel containers, more especially those made of hardened and tempered steel: a power not possessed by oil or water, and which caused them to burst at much lower pressures than when they were charged with oil or water. The phenomenon he attributed to the fact that mercury has the power of dissolving small percentages of iron, and will amalgamate with it when the surfaces are absolutely free from oxide.

In 1912 Bridgeman published his remarkable researches on water under pressures up to 20,000 atmospheres. He found that there are four allotropic forms of ice besides ordinary ice, which are found under various conditions of pressure and temperature with determinate regions of stability. All these forms, except ordinary ice, are more dense than water. One of these forms is remarkable as existing from a temperature of $-18°$ C. under a pressure of 4500 atmospheres up to temperature of 67° C. under a pressure of 20,000 atmospheres.

Recently a pressure of from 200 to 1000 atmospheres at a temperature between 500 and 700° C. has been applied to compel hydrogen to combine with nitrogen to form ammonia on a great commercial scale: a calylist being necessary to promote the combination and to establish the equilibrium between the gases and their product. This action is reversible as regards temperature and pressure. On the other hand, iron just molten is an energetic calylist in the transformation of diamond into graphite, but contrary to expectations, as we shall see, no amount of pressure that has as yet been applied appears to have caused a reversal of this action.

More than thirty years ago, having suitable apparatus at hand, I made a few experiments to try the effect of high pressures and

temperatures on carbon, compounds of carbon and some other substances.

The apparatus consisted of an 80-ton press under which suitable containers were placed, and a turbo-generator of 24 kilowatts output at 80 volts provided the current. It had been discovered by Cheesborough that the carbon filaments for incandescent lamps became very hard and resilient when heated in a hydrocarbon atmosphere of about ½ H_g absolute pressure, and I was anxious to try what would be the result if a rod of carbon were electrically heated when submerged in a liquid hydrocarbon under high pressure. Benzine, paraffin, treacle, chloride and bisulphide of carbon were tested under a pressure of 2200 atmospheres, or about 15 tons per sq. inch. The results were not successful in producing a hard coating to the rod, or of increasing materially its density and hardness, except in the case of tetrachloride of carbon, which slightly consolidated and hardened it; on the contrary, the carbon deposited from the liquids always appeared as soft amorphous carbon like soot. These experiments were extended by substituting instead of the liquids mentioned, silica, alumina and other substances, and increasing the pressure to 30 tons per square inch. When the current density was sufficiently increased the rod was converted to soft graphite. Moissan, in 1903, expressed the view that iron in a pasty condition was the matrix of the diamond, and that great pressure was the determining factor, which compelled a minute fraction of the carbon present to appear as diamond. He further speaks of the probability of carbon being liquefied when under a pressure sufficient to prevent its volatilisation, and that from the liquid state it may pass into the crystalline form on cooling. Crookes, in his lecture delivered before the British Association at Kimberley in 1905, emphasized the same view as to the probability of the crystallization of carbon directly from the molten state on cooling.

Though my original experiments in 1888 were not favourable to these views, it seemed however desirable to carry the investigations up to the greatest possible pressures attainable. Experiments were consequently resumed in 1907 with a new equipment, which consisted of a 2000 ton hydraulic press and a storage battery of 360 kilowatts output. The battery can be coupled for 2, 4, 8, 16, 48 volt as required, and the mains and the main switch can carry currents up to 80,000 amperes to the hydraulic press, which is placed by itself in a small strong house, partly below ground, with walls of 2 feet thickness reinforced with steel bars; the door is of 3 inches thick, the roof is of light galvanised iron. The container under the press is further enclosed by 2 inch thick telescoping steel rings, raised into position by steel ropes and counterweights. These precautions, as experience showed, were necessary, as several violent explosions occurred which cracked the steel rings and blew off the roof. A charge of iron and carbon when confined and raised to

a high temperature may be very violent if suddenly released by the melting of the pole pieces: also some endothermic compounds have been formed which swelled the container and allowed the contents to escape.

My experiments confirmed the conclusion at which Threlfall had independently arrived, that under pressures up to 100 tons per square inch, and very intense heating by electrical current, graphite is not materially changed. But modifications in the experiments were made, and other methods adopted, as will be explained, which in some respects carried the investigation to still higher pressures and temperatures; these however lead to the same conclusion.

This evening I propose to deal to some extent with the practical or engineering side of the subject, and to review the limits of pressure and temperature which are artificially attainable, and to make some comparison between them and the pressures and temperatures occurring in nature.

When the blade of a knife is pressed strongly against another blade so as to make a dent in each, the pressure on the boundary surface of the metal at the notch will have averaged from 300 to 350 tons per square inch, according to the hardness of temper of the steel. The pressures on the knife edges of a weighing machine when fully loaded are also of the same order.

When a needle is broken, or a piece of piano wire is strained to the point of breaking, the maximum tension on the metal will be at at the rate of 150 tons per square inch.

On the other hand, the pressures that occur in the chambers of large guns do not usually exceed 20 tons per square inch, and the tensile stress on the plates of a ship in heavy weather should not exceed 8 tons per square inch.

From these simple instances some idea is gathered of the limitations imposed by materials and dimensions upon apparatus for experimenting at high pressures because of the practical difficulty of hardening and tempering steel in large masses.

When dealing with small amounts of material in each experiment the dimensions allow of the container and the ram being made of tungsten steel, which is a material that can be hardened and tempered throughout, and not only superficially as in the case of ordinary carbon steel. The material is hard and strong, but not brittle, and it retains these qualities up to moderate temperatures, such as 600° C., to a much greater extent than any other steel. Fig. 1 shows a container or die the bore is $1\frac{1}{2}$ inches in diameter, and it may be used for a limited number of times for a pressure of 200 tons per square inch. It will however eventually crack if this pressure is many times repeated, the cracks usually beginning near the bottom of the die.

For still higher pressures it is better to use a double re-entrant container (Fig. 2), with two rams, $\frac{1}{2}$ inch in diameter: both the container and the rams are made of hardened and tempered tungsten steel,

and are made fluid and gas tight by mild steel cups on the ends of the rams.

If the charge occupies only a short length of the bore as shown, the barrel of the container where the charge lies is supported by the sheer strength of the metal above and below the zone of pressure, in addition to its own strength as a tube; under these conditions it is as strong or stronger than the crushing strength of the rams, and pressures of 300 tons per square inch may be repeated several times without cracking.

In a container of this form 7 grains of fulminate of mercury have been placed, imbedded in graphite, and the pressure increased very gradually till it reached 230 tons per square inch (under this treatment fulminate does not usually detonate); the die was then heated by gas to over 180° C., the temperature of detonation. After two failures of the experiment, owing to the leakage of the steel cups, the third was successful and no gas escaped, and the container was uninjured.

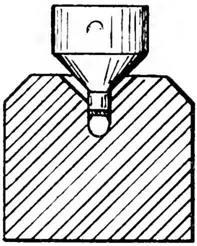

Fig. 1.

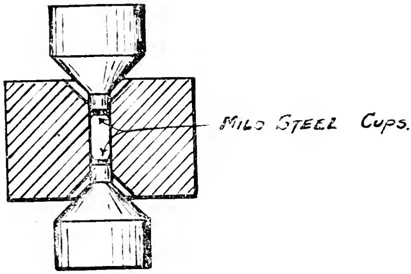

Fig. 2.

The graphite was somewhat caked, but otherwise unaltered. Graphite mixed with sodium nitrate and fulminate was also exploded under the same conditions.

Graphite with 15 per cent. of potassium chloride detonated when 200 tons per square inch had been reached.

Many other reactions were tested in a similar manner in larger dies under pressures of from 40 to 200 tons. The action of concentrated sulphuride acid on sugar was accelerated by a pressure of 50 tons; but on the whole these experiments in dies failed to produce any interesting results.

Unfortunately, the heating of the die with its charge cannot be carried much above 500° C. without seriously weakening the strength of the steel and compelling a reduction of pressure. The electrical heating of the charge in such small dies while keeping the die cool presents great difficulties in electrical insulation on so small a scale to withstand such intense pressure, but I think that it might be accomplished in certain instances.

It has been suggested with the object of reaching higher pressures that a small die might be bodily immersed in a large container; doubtless it could be arranged, but it would be very cumbersome to work with, and would only add about 100 tons per square inch to the maximum pressure.

A better plan would be to follow the principle of the usual capped armour-piercing projectile, and to reinforce the rams and ends of the container by tightly fitting copper or bronze rings around the necks of the rams, keeping the parallel part of the noses as short as possible (Fig. 3).

When in operation the copper rings will be flattened and squeezed against the necks and shoulders of the rams, and also against the ends of the container, and by this means the parts that ordinarily would have to bear the maximum stress will have part of this stress transferred to other parts not so heavily stressed, and consequently

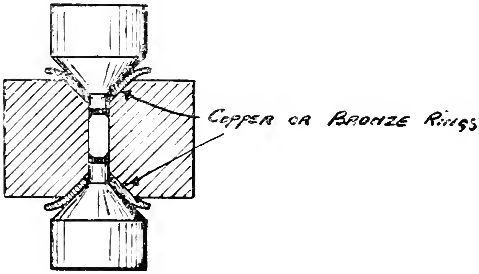

FIG. 3.

the maximum pressure in the container can by this means be raised considerably, perhaps to 450 tons per square inch.

In carrying out experiments on larger samples of material, and in applying electrical heating to the charge, the container becomes too large to permit of its being made of hardened steel; therefore nickel steel is used, as for the barrels of guns. It is heat-treated by quenching in oil from a high temperature after rough machining. Containers (Figs. 4 and 5) with the thickness of wall equal to the diameter of the bore will stand an internal pressure of 40 tons per square inch repeated almost indefinitely without serious enlargement of the bore; but 100 tons necessitates reboring and the fitting of new packing to the ram after each experiment.

Fig. 4 shows the arrangement for electrically heating conductors immersed in fluids under high pressure. The packing of the ram is a cup, leather, backed by a cup of brass; the leather first takes the pressure and the lip of the brass cup is thereby expanded tightly against the bore of the container and remains fluid-tight, even though the leather should be carbonized by the heat. The bottom

pole is electrically insulated from the container by vulcanized fibre washers and a rubber cup ring, which is protected from the heat by magnesite stemming.

The current is conveyed from the container to the top pole piece of the conductor by pads of copper gauze which can slide easily against the bore of the container and allow for the expansion of the conductor. Experiments on liquids with this container under 4400 atmospheres gave the same results as my former experiments under 2200 atmospheres.

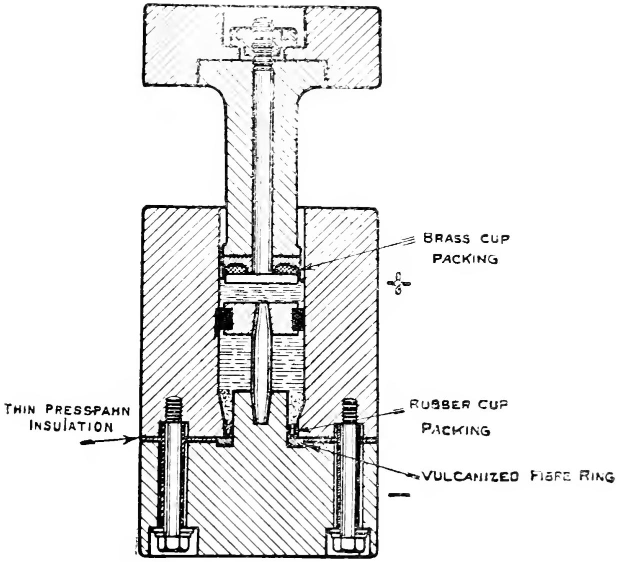

FIG. 4.

Fig. 5 shows the container arranged to melt graphite under pressure by resistance heating. Here the charge is graphite, and is divided by the bridge or ring made of pressed calcined magnesia or of titanium oxide. The bore of the container is electrically insulated from the graphite by layers of asbestos millboard and mica.

The calories evolved in the combination of graphite and oxygen are about half of 1 per cent. less than those evolved in the combination of diamond and oxygen, indicating that graphite at ordinary temperature is to this extent the stable state. The bulk pressure which has operated in some of the experiments would, however, seem to have been amply sufficient to turn the balance in favour of diamond instead of graphite. The uncertainty, on the other hand, as to the compressibilities and specific heats of the allotropic forms of carbon

under high pressures and at high temperatures renders speculation of little value as to what may occur at the melting point of carbon. All we know is, that up to the pressures and temperatures reached in our experiments no indication of a change from graphite to diamond has been produced. In one experiment very intense heating was applied for five seconds, but sufficient in amount to melt the graphite core six times over, the only result being a slight alteration of the structure of the graphite; the barrier in this experiment was calcined magnesia, and the hole in it was superficially converted to magnesium carbide. It appeared, however, desirable further to investigate the possibility of carbon losing its electrical conductivity when approach-

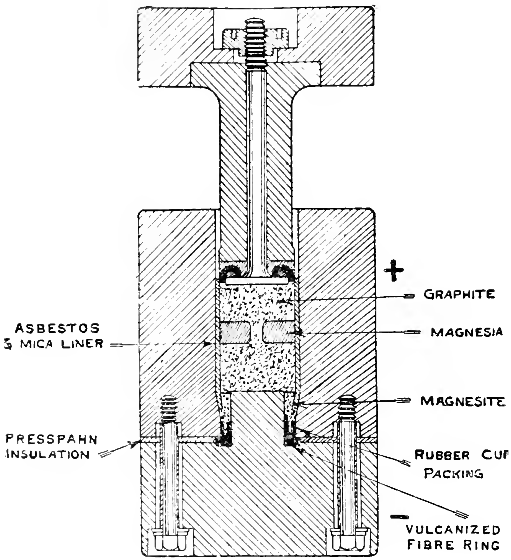

Fig. 5.

ing its melting point, as alleged by Ludwig and others, and of shunting the current from itself on to the contiguous molten layers of the insulating barriers surrounding it. There had been no indication of such a change having occurred, even momentarily: it rather seemed that the graphite core had been partially vaporized and condensed in the cooler parts of the charge. The experiment was repeated with rods of iron and tungsten embedded in the core, so that should the temperature of volatilization of the metals under a pressure of 15,000 atmospheres exceed that necessary to liquefy carbon under the same pressure, the presence of these metals might produce a different result. No change however occurred.

Note.—The temperature at which carbon, iron or tungsten volatilizes under a pressure of 15,000 atmospheres are unknown, but they are probably much higher than at atmospheric pressure.

This experiment also tested iron as a solvent of carbon, and as a calylist from diamond to graphite under a pressure of 100 tons, and showed that under this pressure that action was not reversed.

Fig. 6 shows the container arranged for treating powders by resistance heating with or without the addition of liquids or gases.

The electric current is conveyed from the container to the upper

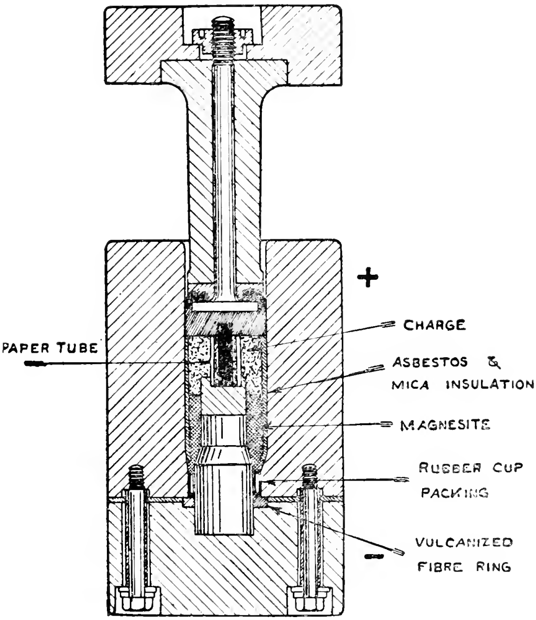

Fig. 6.

end of the conductor by a layer of graphite which rests on the charge under treatment. The bottom end of the conductor rests on or is spigoted into a cast-iron block which rests on the bottom pole; this block is sometimes partially melted, but can be easily renewed. The container is charged by first stemming magnesite powder by hand around the bottom pole piece and block; then the charge is placed on the top and pressed to 5 tons per square inch; the top ram is then removed and a hole drilled through the charge and the container inserted; liquids if used, or carbon dioxide snow, may then be

introduced: lastly, a layer of graphite is placed on the top and the whole pressed to the desired pressure for the experiment.

In one experiment several pounds of carbon dioxide snow were added to the charge, which consisted of magnesia, and was so arranged that evaporation of the heating carbon rod took place in an atmosphere of carbon dioxide and carbon monoxide under a gaseous pressure of 4400 atmospheres, the condensate resulting being soft graphite. Upwards of 200 chemical reactions arranged to deposit carbon were tested under high pressure and central heating. After each experiment samples were taken from various parts of the charge and carefully analysed for diamond, the methods of the analyses generally following those of Moissan and Crookes. Very small residues of diamond occasionally occurred, but they

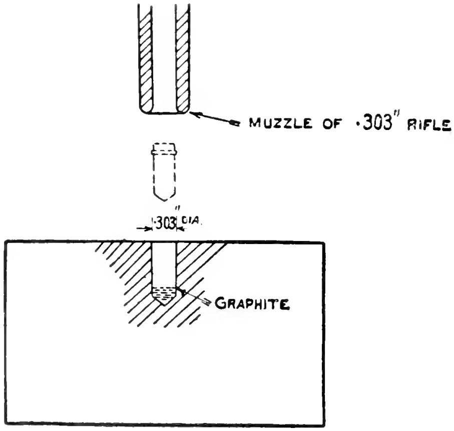

Fig. 7.

appeared to be associated with the presence of iron in the charge, whether introduced intentionally or from the melting of the pole pieces, short circuits, or from other causes. On the whole there was no evidence that diamond had been produced by any of the chemical reactions, some of which were endothermic, such as carborundum and sodium carbonate, which produced a grey solid which detonated when struck with a hammer, and nearly caused a serious accident. In one experiment the charge was olivine and water; when molten under 10 tons per square inch, the pressure was suddenly removed and artificial pumice was formed by the expansion of water vapour absorbed by the olivine when molten. Having nearly reached the limits of steady pressure obtainable in steel containers under a press, experiments with impact pressures produced by steel bullets were

tried which produced much higher instantaneous pressures than are obtainable in any die.

A rifle 0·303 inch bore was arranged for withstanding a charge of cordite 90 per cent. in excess of the service charge.

The gun (Fig. 7) was fixed with its muzzle 6 inches from a massive block of steel, in which a hole 0·303 inch in diameter had been drilled to a depth somewhat greater than the length of the bullet, and in alignment with the bore of the gun: cylindrical bullets of steel with a copper driving band were chiefly used, shorter than the service bullet, and about one-half of the weight. The substance to be compressed was placed either at the bottom of the hole, when a conical-nosed bullet of mild steel was used, or over the

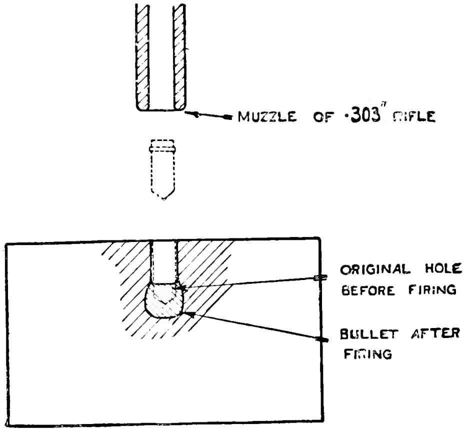

Fig. 8.

mouth of the hole, when a cupped-nose bullet of tool steel was employed. About one hundred experiments were made.

The substances tested included graphite, sugar carbon, bisulphide of carbon, oils, etc., graphite and sodium nitrate, graphite and fulminate of mercury, finely divided iron and fine carborundum, olivine and graphite, etc. After each shot the bullet and surrounding steel were drilled out, and the chips and entrained matter analysed. Fig. 8 shows the bullet in the hole after firing.

Several experiments were also made with a bridge of arc light carbon placed over the hole and raised to the limit of incandescence by an electric current, and the shot fired through it into the hole at the moment the carbon commenced to vaporize, as observed in a mirror from without. Also an arc between two carbons was arranged to play just over the hole, and the shot fired through it (Fig. 9)

The residues were in all cases exceedingly small, and there was no evidence of any incipient transformation of carbon in bulk into diamond that could be detected by analysis.

The pressure on impact of a steel bullet fired into a hole in a steel block which it fits is limited by the co-efficient of compressibility of steel; with a velocity of 5000 feet seconds it is about 2000 tons per square inch. Measurements made from a section through the block and bullet (Fig. 8) showed that the mean retardation on the frontal face after the impact till it had come to rest was about 600 tons per square inch. Several experiments were made by substituting a tungsten steel block hardened and tempered and a hole tapering gently from 0·303 inch at the mouth to 0·125 inch at the bottom. The mild steel bullet was deformed by the tapered hole, which greatly increased the velocity imparted to the nose. Progressively increased charges were used. With the 90 per cent. excess charge the block always split on the first shot, but this probably

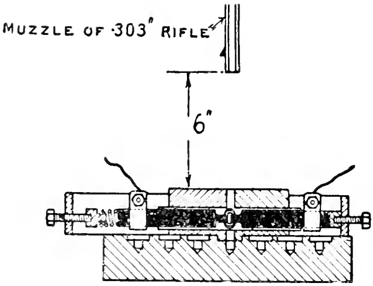

Fig. 9.

occurred after impact, and not till the full instantaneous pressure had been exerted, which was probably about 5000 tons per square inch, or about equal to that at the centre of the earth.

It would be interesting to repeat some of these experiments on a larger scale. With a projectile of 6 inches or 9 inches in diameter and a velocity of 5000 feet seconds, the instantaneous pressure would be the same, but its duration (which is proportional to the linear dimensions) would be increased from 20 to 30 fold. It has been estimated that the rise in temperature due to adiabatic compression of incandescent carbon when subjected to 2000 tons per square inch is of the order of about 1000° C., so that actual melting of the carbon would probably have occurred when the shot was fired through the incandescent carbon bridge.

Another experiment was arranged which would ensure that carbon should be subjected to an extremely high temperature concurrently with high pressures, obtained by the rapid compression of the hottest

possible flame, that of acetylene and oxygen, with a slight excess of the former to provide the carbon. The arrangement was as follows (Figs. 10 and 11):—A very light piston made of tool steel was carefully fitted to the barrel of the gun of 0·9-inch bore: the piston was flat in front, lightened out behind, and fitted with a cupped copper gas check ring, the cup facing forward: the total travel of the piston was 36 inches. To the muzzle of the gun was fitted a prolongation of the barrel formed out of a massive steel block, the joint being gas-

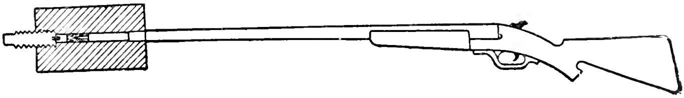

Fig. 10.

tight; the end of the bore in the block was closed by a screwed-in plug made of tempered tool steel, also with a gas-tight collar. A small copper pin projected from the centre of the plug to give a record of the limit of travel of the piston. The gun was loaded with 2 drachms of black sporting powder, which amount had been calculated from preliminary trials. The barrel in front of the piston was filled with the mixture of acetylene and oxygen. It was estimated

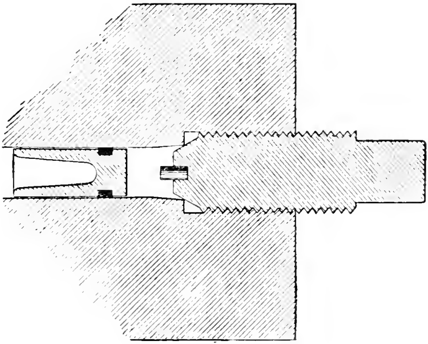

Fig. 11.

that this mixture would explode when the piston had travelled about half way along the bore: when fired the piston travelled to within $\frac{1}{8}$ inch of the end, as had been estimated, giving a total compression ratio of 288 to 1. As a result it was found that the surfaces of the end plug, the fore end of the piston, and the circumference of the bore up to $\frac{3}{8}$ inch from the end of the plug had been fused to a depth of about 0·01 inch, and were glass hard: the surface of the copper pin had been vaporized, and copper sprayed over the face of

the end plug and piston. The end plug, which had been hardened and tempered to straw colour, showed signs of compression, and the bore of the block for $\frac{3}{8}$ inch from the plug was enlarged by 0·023 inch in diameter, both indicating that a pressure above 100 tons per square inch had been reached.

A little brown amorphous carbon was found in the chamber, which was easily destroyed by boiling sulphuric acid and nitre. There was no diamond residue from this. Considering the light weight of the piston and the very short duration of the exposure to heat, the effects would indicate that a very abnormal temperature had been reached, many times greater than exists in the chambers of large guns. A calculation made by Mr. Stanley Cook, based upon the ratio of compression and a final pressure of 15,000 atmospheres, indicates that a temperature of between 15,250° C. and 17,700° C. was reached, the exact temperature depending upon the amount of dissociation or combination existing between the elements at the time.

Calculation of the Temperature Reached on the Compression of Acetylene and Oxygen Experiment.

By Stanley S. Cook.

The temperature reached may be estimated from the final pressure, which the observed deformation of the block and plug indicates to have been in the neighbourhood of 100 tons per square inch. But it must be remembered that there is a change of molecular volume as a result of combustion. Thus the mixture, which as C_2H_2 and 5 (O) has $3\frac{1}{2}$ molecular volume, would on combustion to 2 CO_2 and H_2O have only 3 molecular volumes. The final temperature deduced from the pressure will therefore depend upon the extent to which chemical combination has taken place.

The original mixture being at atmospheric pressure and a temperature of 290° C. absolute, a pressure of 100 tons per square inch after compression to $\frac{1}{2\,2\,8}$ of its original volume would indicate a temperature of 15,250° C. If, however, complete combustion has taken place, this same pressure would correspond to a temperature greater in ratio of $3\frac{1}{2}$ to 3, viz. to 17,700° C. The actual temperature must therefore have been something between these two values.

Let us for a moment consider the pressures and temperatures possible in nature (in this I am indebted to kind assistance from Professor Jeans). The pressure at the centre of the earth is between 4000 and 10,000 tons per square inch, according to the variation in density of the concentric layers.

Emden has estimated the probable pressure at the centre of the

more massive component of the binary star S. Hercules to be 360,000,000 tons per square inch.

Again the densities of the brighter components of the stars of Cassiopeiæ and B 612 are estimated by Opik to be about that of iron; and if we assume their diameter to be the same as that of the sun, and that each has an initial velocity in space not greater than 30 miles per second, and that they directly collide, then, owing to their mutual attraction, Jeans calculates that their velocity will have increased to 450 miles per second, and the pressures in the centre as they strike and flatten would be of the order of 1,000,000,000 tons per square inch. He also estimates that the heat equivalent of the energy would be sufficient to vaporize the whole mass 100,000 times over. This immense pressure would be maintained for many minutes, perhaps for half an hour.

Let us consider what is the greatest pressure that can be produced artificially. If the German gun which bombarded Paris were loaded with a solid steel projectile, somewhat shorter and lighter than the one actually used, a muzzle velocity of about 6000 feet seconds might be reached (many years ago Sir Andrew Noble had reached 5000 feet seconds): and if it was fired into a tapered hole as I have described, in a large block of steel, this would give the greatest instantaneous pressure that can be produced artificially, as far as we at present know, viz. about 7000 tons per square inch. This is only about 1–150,000 part of that possible by the collision of the largest stars.

As to the temperature and conditions of matter under these intense pressures exterpolation from known data is valueless. We have no knowledge of the co-efficients of compressibility of matter under these conditions, or of its specific heat—what may be the effect on the atom, and will elements under such conditions be transformed into others of higher atomic weight?

Some of us may recall that in 1888 the lecturer, after describing in this room the experiment in which oxygen at atmospheric pressure was passed in close contact with a platinum surface heated by the oxyhydrogen burner to nearly its melting point, and then immediately cooled by contact in water, said: "In this experiment ozone is formed by the action of a high temperature, owing to the dissociation of the oxygen molecules and their partial recombination into the more complex molecules of ozone. We may conceive it not improbable that some of the elementary bodies might be formed somewhat like the ozone in the above experiment, but at very high temperatures, by the collocation of certain dissociated constituents and with the simultaneous absorption of heat."

It seems indeed probable that the centres of the great stars and stars in collision may be the laboratories where the elements as they gradually degenerate are being continually regenerated into others of higher intrinsic energy, and where endothermic processes such as the

recombination of lead and helium into radium may be taking place, absorbing in this process an energy of $2\frac{1}{2}$ million times that developed by the explosion of an equal weight of T.N.T.

The transformation of only a minute fraction of the mass of two colliding stars would therefore be amply sufficient to absorb the whole energy of their collision.

Emerson said many years ago, "None but the elements can themselves destroy."

[C. P.]

WEEKLY EVENING MEETING.

Friday, January 30, 1920.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sidney G. Brown, F.R.S. M.I.E.E. M.R.I.

The Gyrostatic Compass.

This lecture is on the Gyrostatic Compass, called in short the Gyro-Compass.

An engineer of my acquaintance was asked if he understood what a Gyro-Compass was, and he replied, "Of course I do ; it is a magnetic compass mounted upon a gyroscope."

Now that is not correct, because the Gyro-Compass has nothing to do with magnetism or the magnetic compass. The only thing that these two instruments have in common is to point towards the north and south poles of the earth.

I am, therefore, anxious that this should be clearly understood, because in a recent lecture I gave at Bournemouth on this very subject, one of the audience asked me after the lecture how I shielded the Gyro-Compass from outside magnetic influence. I pointed out to him, as I had endeavoured to do during the lecture, that the Gyro-Compass had nothing to do with magnetism, and, therefore, did not require shielding. He then asked me what worked the compass. I said it received its directive action from the rotation of the earth ; this I fear was too much, for after looking at me reproachfully he moved away.

I, therefore, want you to get firmly fixed in your minds, that the magnetic compass and the Gyro-Compass are two absolutely different instruments operated by entirely different laws, although they are for the same purpose.

I have often been asked why a Gyro-Compass is needed when the magnetic compass is already available, and I therefore feel it necessary to say a few words on the magnetic or mariner's compass before attempting to explain the Gyro-Compass.

The mariner's compass consists of a magnetic needle, or of several magnetic needles fixed side by side, and balanced upon a sharp point.

A card divided into 32 (points of the compass) is attached to the needle, and swings round with it, so that the point marked N on the card always points to the north.

The earth, as we know, is a magnet, but not a very powerful one, and it has been calculated that if it were wholly of iron, it would have an intensity of magnetism 17,000 times greater than it has. All the same the magnetism is sufficiently strong to give a good directive action to a pivoted needle.

The magnetic poles of the earth are not coincident with the geographical poles, but are situated some distance away. The magnetic pole was discovered by Sir J. C. Ross to be situated in latitude 70° 5′ North and longitude 96° 46′ West in Boothia Felix, just within the Arctic Circle some 1,000 miles away from the actual pole.

With this displacement of the magnetic poles, we have an irregular distribution of the magnetism over the surface of the earth, and thus the magnetic needle does not point truly north and south at many parts of the earth's surface. In London, for instance, it points at an angle of 16° west of the true north.

This angle is called the deviation or variation of the needle. To enable ships to steer by the compass, magnetic charts have been prepared, and the deviation at different places accurately measured.

These magnetic charts have to be checked and altered from time to time, as the deviation slowly varies from year to year. Thus in London in 1659 the needle pointed true north, while in 1820 there was an extreme westerly variation of $24\frac{1}{2}°$; since then it has been slowly coming back to something like 16° at the present time.

On a wooden ship the accuracy of a good modern magnetic compass leaves little to be desired, but on an iron ship the case is quite different.

The magnetic field of the earth tends to be weakened in the lengthwise direction of the iron ship, because a portion of the magnetism enters the ship, while across the ship the field is stronger, and as it is essential that the magnetism in which the needle lies should be uniform in strength in whatever direction the ship may happen to point, it is important that this stronger field be reduced by some method of magnetic shielding. This is accomplished by fixing a pair of iron globes athwart the ship on the two sides of the compass. The effect of the iron of the ship and the corrections that have to be made to the compass is to reduce the directive force of the earth's magnetism, and thus the compass is rendered slow and sluggish in its action. This is particularly the case on board a battleship.

In the interior of a submarine the force is still further reduced, so much so as to render the magnetic compass useless for this class of vessel.

It is quite possible on an iron ship to correct the errors of a compass, but as the ship itself may be a magnet, and its strength a variable quantity, it is important that the navigator should test the readings of his compass at every available opportunity, and particularly at the commencement of each voyage.

The ship's magnetism may quickly change through the hammering action of the waves, or the heating action of the sun on one side of the vessel, or through an earth on any of the electric wires that may be running near the compass; all these things together add to the anxiety of the captain, as he is never quite certain how far the compass is correct in its readings. The swings of the modern

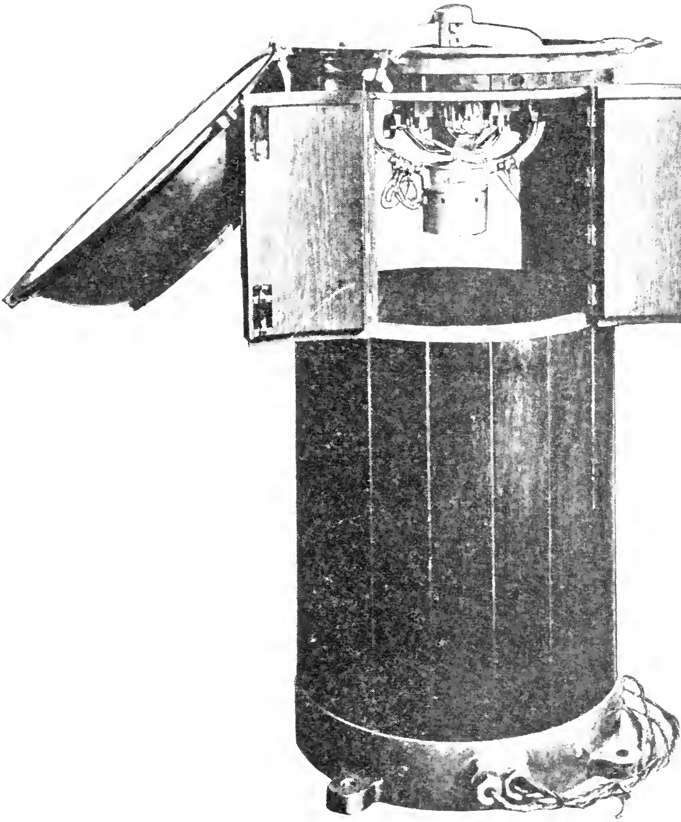

FIG. 1.—GYRO-COMPASS IN BINNACLE.

compass are damped by immersing the needles and card in a liquid such as alcohol, but as this fluid is attached to the ship and turns with it, swinging the ship in any direction carries the liquid round and re-acts on the needle and card, so that the compass has a tendency to be carried round with the vessel.

This lag in the instrument renders it difficult to hold a ship dead on her course, and the path, as a consequence, is sinuous, and may oscillate, even in a calm sea, as much as 7° each side of the correct heading.

As a ship has usually to steam entirely by the readings of the compass any error is serious. For instance, if there is an error of 3°, and the ship is steaming at 16 knots, she will move one English mile off her course every hour. It is obvious how necessary it is to have absolutely correct readings. Lord Kelvin was the first to seriously study the errors of the magnetic compass. He started in 1871, and in 1876 produced his well-known instrument. Although it was a great advance on any compass in the British Navy, he had the greatest difficulty to get them to adopt it ; finally, in 1879, he proposed to place an instrument at the disposal of the Admiralty at his own expense. This offer was accepted. In spite of this, it was only through the acquaintance of influential Naval officers, particularly of Captain (now Lord) Fisher, that the compass was ever adopted.

In 1889, eighteen years after the commencement of his experiments, and long after it was in common use in commercial ships, he received official notification that his 10-inch compass was to be adopted in future as the standard of the Navy. It is fortunate that we have an alternative method of securing a north-seeking property in the Gyro-Compass, an instrument of much greater accuracy than the magnetic, and with none of these errors, for if deviations do occur they are known deviations, and can therefore be allowed for.

Evans and Smith, in 1861, were the first to discover how important it was to mount the needles on the card so that the moments of inertia of the moving system should be the same about all directions—that is to say, that the system should be in dynamic balance—otherwise the rolling of the ship would cause deviations in the reading. I have lately discovered that another deviation may be brought about, not by an oscillation in one direction, but by the card being set wobbling ; the needles and card would then have a force applied trying to carry the moving system round in the direction of the wobble.

I have a magnetic compass here to demonstrate this. It consists of a heavy brass disc mounted on a vertical frictionless spindle. The needles are fixed to the disc, and the whole movable system is carried on a pendulous mounting, as in the Gyro-Compass. The disc and needles are in correct static and dynamic balance.

Swinging the pendulum in any one direction produces no deviation, but by causing it to swing in a circular conical path, thus giving a wobble to the plate, we have a serious deviation in the reading of the compass : the error is permanently maintained against the earth's attraction so long as the circular motion of the pendulum persists.

I have also carried the compass round in a horizontal circular

path, without wobble, and I find that the plate still goes round with the circular movement.

This should be of interest to the mathematicians.

Before I leave the instrument I will set it spinning, so as to demonstrate to you the frictionlessness of the vertical axis.

It is rotating now entirely by means of the energy of the motion of the plate, and I think you will find at the end of the lecture that it is still revolving, but of course not so fast as at present.

The magnetic compass is a simple piece of apparatus, but it is

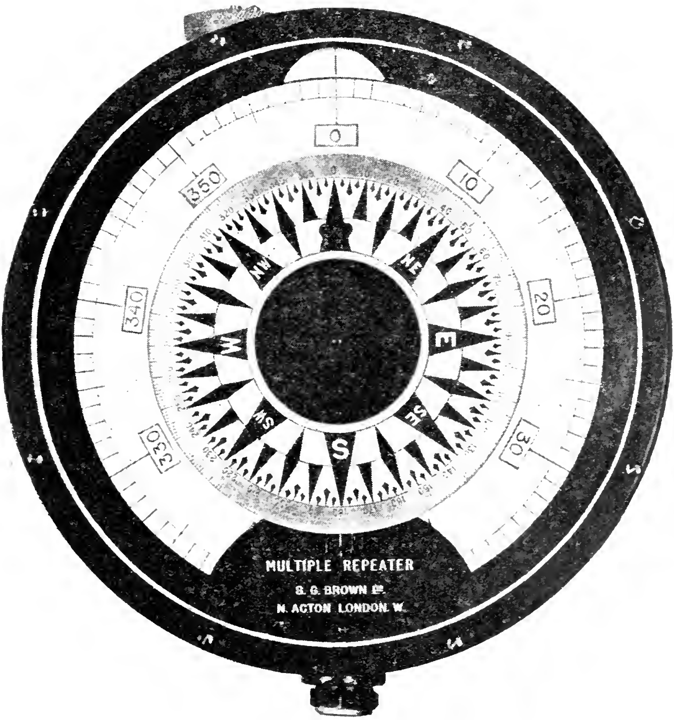

FIG. 2.—STEERING REPEATER.

complicated in its readings and corrections, and points to the magnetic north. The Gyro-Compass is a complicated instrument, but is simple in its readings and points to the true north.

Before proceeding to describe the Gyro-Compass, I had better say a few words about the equipment in front of you. The Gyro-Compass is in full operation, and is at the present moment recording its movement upon a travelling strip of paper.

Some half hour before the lecture started, the compass was deflected from the north position, and it has since been left to itself. You will see by the record that it is engaged in swinging back again to the north, recording a curve upon the paper strip.

You will be able to follow the record in this way during the whole of this lecture, as you cannot really look down upon the compass itself.

The compass is working two repeaters, these repeaters truly copying the reading of the master compass.

Of course, any number of repeaters could be used on board ship if it were necessary.

Fig. 2 is the steering repeater; it has a card that revolves four times to one of the master, and the divisions are therefore very much enlarged.

The other repeater is a correction repeater, which I shall tell you about later; it has no magnified scale; it is moving backwards and forwards very slightly, and this motion we term the "hunt." In the steering repeater the "hunt" has been cut out by providing the mechanism within the case with a requisite amount of slackness.

About sixty-eight years ago Foucoult did what was thought a wonderful thing at the time: he gave a lecture-room proof that the earth was rotating on its axis—he looked through a microscope at a gyrostat. He could not get a frictionless free vertical axis, so that the experiment could not last for long.

Presently I shall show you a piece of apparatus which carries out Foucoult's idea in a very perfect way, and it will be visible to this audience.

Of course we all know now that the earth rotates, and that this force of rotation is utilized to work the Gyro-Compass, but it was about 1616 that Galileo first pointed out the fact, contrary to the Copernican theory, that it was the earth that rotates and the sky that stood still.

He was tried for this before the Inquisition, and, although he withdrew his statement, he is said to have risen from his knees and stamped on the ground, exclaiming, "Yet it does move!"

I shall now indicate a few of the laws of the simple gyrostat, so that the descriptions of the instrument may be better understood.

A gyrostat consists of an accurately balanced spinning wheel, mounted with as little friction as possible, and in such a way that the axis of the wheel may point in any direction in space.

Mere translation in space has no action on the instrument; carrying it about, for instance, does not alter the direction of the axis.

On the other hand, the gyrostat is acted upon by any force that tends to tilt the axis, or to give the axis a new direction in space.

The wheel is spinning round its axis. Call the direction of this

OA. If we impress a force upon the wheel, tending to tilt or rotate it round another axis OB, then the rule is that the spinning wheel will "precess" or move in such a direction as to try to make the two axes OA and OB coincide, and the direction of spin of the wheel to coincide with the new direction of rotation that we are trying to produce by the applied force.

On the right-hand side of Fig. 3 I have indicated an electric circuit which has similar mathematical laws to that of the gyrostat. It consists of an outer fixed coil and a central suspended coil.

A strong direct current, indicated by *a*, is kept flowing in the central coil; this corresponds to the spin of the wheel.

If a direct current, indicated by *b*, is sent round the outer coil, then the central coil will move in such a direction as to make the axes of the magnetic fields of the two coils coincide, and to make the direction of the two currents also coincide.

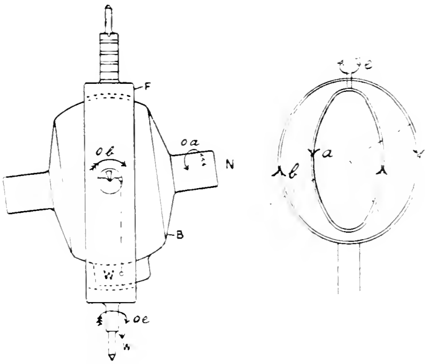

FIG. 3.—GYROSTAT AND ELECTRICAL CIRCUIT EQUIVALENT.

In fact, the coils will move, or try to move, in such a way as to make the self-induction of the whole circuit a maximum.

This is very much like the gyrostat (or, in fact, any piece of mechanism), which under impressed forces tends to move so as to make the whole moment of inertia a maximum.

Suppose, therefore, a gyrostat has its axis OA fixed parallel to the earth's surface, but free to turn in "azimuth," as it is called, upon a frictionless vertical spindle, the earth will act upon such an instrument, and it would be a Gyro-Compass.

The earth as it rotates is continually tilting the axis of the wheel in space; the wheel will, therefore, turn so as to set its axis of rotation as nearly as possible parallel to the axis of the earth. It is only when the two axes coincide that the wheel is free of any further tilting action—that is, when it is pointing true north; deviate the

axis, however little, from this position of rest, and the action of the earth comes in again to precess the wheel back again to the north.

I have here a simple form of gyrostat with three degrees of freedom. If I hold it in my hand and revolve on my axis it does not move the wheel, which still keeps pointing to the same part of the room.

On the other hand, if I restrain or clamp one of its degrees of freedom so that I am able to tilt the axis of the wheel during my revolution, I cause the wheel to precess and to set its axis parallel to the axis on which I am revolving. Reversing the rotation, the wheel also reverses.

This is what takes place with the gyrostat on the earth's surface, provided it is frictionlessly mounted. I have here such an instrument, and I will try and demonstrate by its means the rotation of the earth.

A wheel is rotating inside this case at 15,000 revolutions per minute.

The case is constrained to move about this vertical frictionless axis.

Mere motion of translation will have no effect in changing the direction of the axis of the wheel, but if this room rotates, the axis of the wheel will tend to set itself parallel to the axis about which the room is rotating.

We all believe that this room is rotating about the axis of the earth; if so, the axis of the wheel will try to set itself parallel to the axis of the earth, but it must be kept horizontal, and therefore it will point north and south.

Here it is pointing in an E. and W. direction; it is held by a string. I will now burn the string, and it will find for us the true north.

Observe that it is really the true north direction, whereas the magnet points to the magnetic north.

I will set it away from the north, but on the other side, and repeat the experiment.

Such a simple form of Gyro-Compass could not be of any use on a moving ship, because the rolls of the ship would react too violently on the spinning wheel and cause considerable deviations in the readings of the compass.

The use of a Gyro-Compass on land is very limited, and its great value at the present time is on board ship.

I have intimated to you that the spinning wheel is acted upon by forces which tilt the axis. Now a rolling and pitching ship is about the worst place to put a gyrostat to act as a compass, because the ship's movements all tend to tilt the axis.

The problem, therefore, is to make the compass insensible to the movements of the ship, and respond only to the slow angular rotation of the earth.

To indicate the severity of the ship's movements I may recall a recent trip of this Gyro-Compass on board a fast destroyer. During a severe gale the ship was recorded to roll over 50° of total angle. Many of the crew were forced to lie on the decks, the lockers emptied their contents, and even some of the oil-lamps suspended from the ceiling were unseated by the pitching of the vessel, and yet the Gyro-Compass maintained its accuracy and allowed the ship to be safely steered into harbour, to which she had to run for safety.

In all this whirlwind of movement the Gyro-Compass heard and only responded to the still small voice of the earth's rotation. How small this force is I shall try to indicate to you a little later on.

For use on board ship the compass must be mounted on a pendulum in gymbal rings, and its period of oscillation is lengthened to something like 85 minutes, which is usual in practice, so that the rolls, which are of the order of 7 to 15 seconds' period, have but small effect on the compass.

In this case the rotation of the earth does not act directly upon the gyro wheel, but by means of the force of gravity through the pendulous weight. Unfortunately this form of mounting introduces troubles of its own.

Suppose we study our simple gyrostat and see what happens when we attach a weight to the end of the horizontal spindle: this will give us some idea of what occurs when the force of gravity is acting through the pendulum trying to tilt the gyro wheel.

We know from our law that the wheel will precess under the tilting action, but as the new direction of rotation that we are trying to produce by means of the weight, unlike that produced by the earth, which is always in one direction, is in this case continually carried round by the precessing wheel, the precession is permanently maintained. We also find that if we hurry the precession the spindle rises, lifting the weight; while, on the other hand, if we delay the precession the spindle drops, and the weight falls. The rate of precession is proportional to the weight; halving the weight, for instance, halves the rate at which the wheel rotates round the vertical support.

Coming back again to our pendulous mounted Gyro-Compass. Suppose the spindle is pointing west, and is horizontal, then the earth as it rotates will leave the wheel pointing in this one direction in space, but the weight will try and follow the earth's rotation, and will start precessing the gyro towards the north. The rate at which the wheel comes to the north depends upon the weight attached to the casing. All the time the wheel is coming to the north the earth is adding to the rate of the precession, and the spindle is as a consequence tilted, and is deflecting the weight at the north position. Under these conditions the effect of the weight is to continue the precession, and the gyro wheel will swing through the north position,

and will continue to move until the effect of the earth arrests and reverses the motion.

The compass will therefore continue to swing through the north position with constant amplitude backwards and forwards, undamped.

To render the compass of use some method of damping the swing must be introduced, so that the compass may finally settle on the north.

This damping can be carried out by means of friction—preferably fluid friction—between the vertical spindle and its support, but although this will damp the swings it is inadmissible, because the movements of the ship would react through the friction and cause errors in the reading.

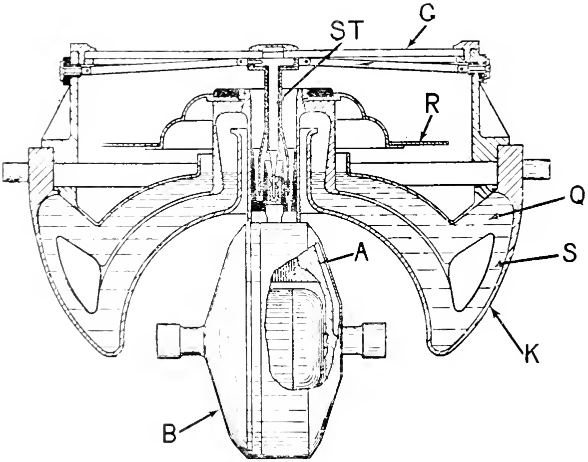

FIG. 4.—ANSCHUTZ EARLY FORM OF GYRO-COMPASS.

Anschutz, in his early form of compass, by use of an air blast gets rid of this connection with the ship. The air blast was arranged to oppose the movement in "azimuth" when the wheel tilted, and thus he obtained an effective method of damping. The strength of the air blast, which varies proportionally to the tilt, should be nothing when the compass is at rest on the north—that is, when the tilt is nothing—and this would be true with the compass on the equator.

In other latitudes, however, the compass rests at the north with a tilt still remaining. It does not come back to the horizontal position because the axis of the wheel is trying to set itself parallel to that of the earth. This leaves a residual air blast continuously

acting, producing a permanent twist in "azimuth" and a constant error. It is therefore preferable to damp the swings of the compass by acting upon the tilt rather than its movement in "azimuth," because in this case there will be no latitude error. The tilt is a maximum at the middle of each swing—that is, when it is moving through the north position—and it is the return of the weight to its truly vertical position that is responsible for the continuation of the

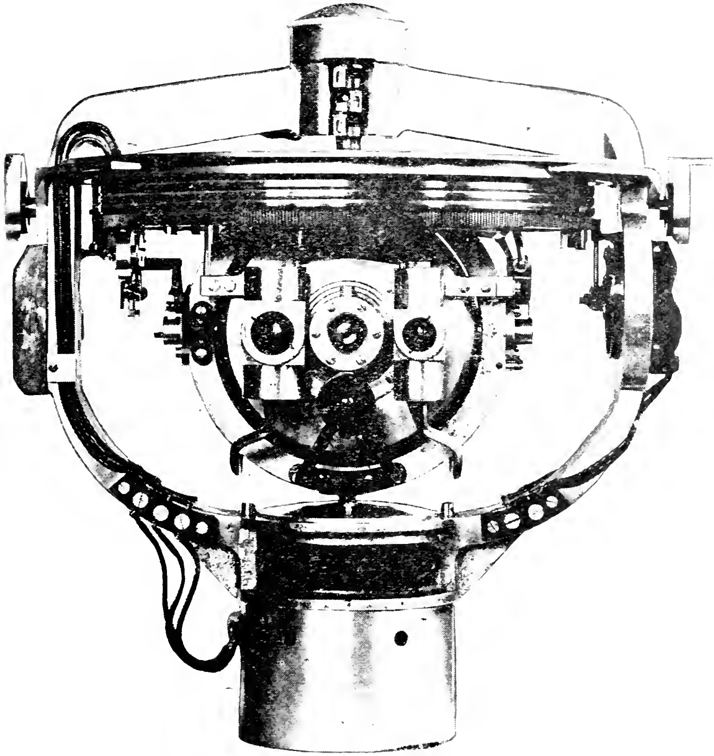

FIG. 5.—"BROWN" GYRO-COMPASS REMOVED FROM BINNACLE.

oscillation. We therefore require some method of neutralising the action of the weight, not before, but after the compass has reached the north. This I accomplish in the "Brown" Gyro-Compass by automatically moving a liquid from one bottle to another, and in such a direction as to counterbalance the weight, precessing the gyro wheel; and I delay its action by means of a valve or constriction in the tube joining the two bottles (see Fig. 10).

The force with which the compass seeks the north is proportional to the product of the rotation (one revolution in 24 hours) and the spin of the wheel. The faster we can spin the wheel the more do we obtain directive force. It is for this reason that the wheel is rotated at its maximum speed and strength consistent with the rise of temperature.

Taking the "Brown" Gyro-Compass as an example, the wheel, which is 4 in. in diameter and $4\frac{1}{4}$ lbs. in weight, runs at 15,000 revolutions per minute. The maximum directive force of the earth on this wheel—that is, when the spindle is pointing east to west—is only the weight of 30 grains, with a leverage of 1 inch.

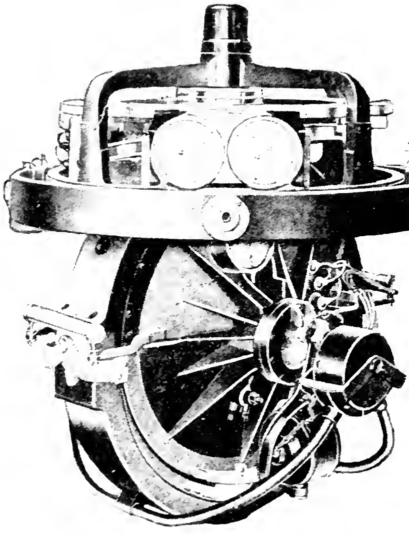

Fig. 6.—Sperry Gyro-Compass.

This small force is continually diminishing in value as the axis approaches the north direction, and vanishes absolutely in that position.

If the compass were deflected, say 1° from the north, then the force of restoration is only $\frac{1}{2}$ grain at a leverage of 1 inch. It will therefore be seen how important it is to eliminate as completely as possible any friction on the vertical axis that would tend to oppose the directive action of the earth.

There are three forms of Gyro-Compass now in use, the Anschutz (German), the Sperry (American), and the "Brown" (British).

In the "Anschutz," Fig. 4, the vertical axis is supported by a

bath of mercury; in the "Sperry," Fig. 6, by a suspended wire, the twist, if any, being taken out by a follow-up motor, through an electric contact which switches on the current to the motor; and in the "Brown," Fig. 7, by a hydraulic system of support. The lower end of the vertical spindle acts as a ram and stands upon a column of oil. The oil is under great pressure, some 500 lbs. per square inch, and is kept pumping up and down, and thus raising and lowering the vertical axis continually some 180 times every minute.

The continual movement of the spindle results in a practically frictionless vertical support, so that the total moving part, some $7\frac{1}{4}$ lbs. in weight, can be carried round in "azimuth" by the smallest force, due to the earth's rotation: in fact, so small is the friction that the compass, if deflected, will always come back again to its true north position, certainly within $\frac{1}{10}$th of a degree. I think I am safe in saying that it is the most perfect frictionless support yet given to the vertical spindle of any Gyro-Compass, or indeed of any machine.

I have mentioned in an earlier part of this lecture that the period of oscillation given to a Gyro-Compass is of the order of 85 minutes. I will now try and explain why this is so.

The earth has no angular movement from south to north, but has one from west to east, due to the daily revolution on its axis.

A ship, however, sailing to the north at, say, 20 knots an hour introduces an angular movement in that direction, because it is moving over the curved surface of the ocean, and would complete a revolution of the globe in 45 days.

If there were a Gyro-Compass on the ship, the instrument would be sensible of these angular movements, and would set itself so as to make a compromise between them, and would as a consequence point, not to the true north, but one or more degrees west of the actual pole; this division is termed the "North Steaming Error."

Knowing the latitude, the speed of the ship, and its direction towards the north and south, the extent of the error can be accurately calculated, and speed correction tables have been prepared so that this error can be determined for any latitude, speed and heading of the ship, and can be allowed for.

Automatic means have also been devised to make these necessary corrections in the reading of the compass. For instance, this, my special form of repeater, has been designed with the card set eccentric, so that, when once set, the correction will be automatically applied without any further reference to the tables.

If a ship is in harbour, then a Gyro-Compass on board would be pointing due north, but when the ship starts steaming to the north, the compass will begin an oscillation so as to bring the axis of the wheel into the new resting position to include the north steaming error into the reading.

Getting up speed will, however, have another effect on the compass. We know that the gyro wheel is acted upon by a pendulous

weight. As the ship changes its speed the acceleration will act upon the pendulous weight and cause an oscillation to be started. This oscillation is termed the "Ballistic Deflection."

The permanent north steaming error and the transitory error, due to the ballistic deflection, are in the same direction, and mathematicians have calculated that with an undamped Gyro-Compass, if the time of its oscillation is set to 85 minutes in any particular latitude, the ballistic deflection can be made exactly the same as the deflection due to the north steaming error; this being so, the compass should move into its new resting place without further oscillation.

This would be true if, as before indicated, the compass was undamped in its swings; but the mathematicians have overlooked the fact that all Gyro-Compasses are damped, and the ballistic deflection must, therefore, include a term due to the damping.

This damping term up to the present has been neglected; but in practice it is found that when a ship is steaming and turning to alter its course, the compass does not come deadbeat to its new position, but has an oscillation started which is common to all existing Gyro-Compasses. The extent of this oscillation may be termed the "Damping Error."

On a merchant ship the damping error is of little moment; but on a war vessel which is manœuvring it may be serious, as it may swing the compass off its correct reading by several degrees.

I have made certain modifications in my compass to remove the damping error, but will not explain my method as it has not yet been tested in practice.

I have drawn attention to several faults that have to be rectified if the compass is to be of use on a ship, and I shall now discuss the last, but by no means the least, of the errors that may arise if the instrument is not properly designed.

This error was not known when the Gyro-Compass was first brought out: it proved a most difficult fault to correct, and its elimination has had more to do with the design of the later forms of Gyro-Compasses than any other factor.

If a gyro wheel is precessed towards and kept pointing to the north by an ordinary pendulum weight it will work well on board ship, provided that the ship is steaming on a fairly smooth sea; but if the direction of the compass points anywhere in the quadrants—that is, N.W. or N.E., S.W. or S.E.—and the ship rolls, the wheel will try to set itself so as to bring the rim of the spinning wheel in line with the roll, and in a long continued and heavy roll the compass may show an error of twenty or more degrees, a most serious fault, and one that would render the instrument quite useless in a heavy sea. This error is called the "Quadrantal Error."

The extent of the error depends upon the violence of the ship's rolling and the direction of the axis of the wheel.

If the compass points direct N., S., E., or W. the error is nothing, but it would be a maximum in any of the directions before mentioned.

I think Anschutz was the first to point out the error and suggest a cure; this I gather from one of his publications in the year 1911, in which, speaking of the tendency of the compass to wander when on board ship, he says:—

"Theoretically the influence of rhythmic turning movements on a gyroscopic apparatus must disappear completely if not only the real, but also the apparent moments of inertia of the movable system become equal for each plane."

If we go back again and study our simple Gyro-Compass, we see that the movable system is not symmetrical. In the direction of the axis of the wheel the effect of tilting movement is more or less resisted by the spinning wheel; this may be termed the stabilized direction, while at right angles to this—that is, in the direction of the rim of the wheel—there is no resistance to tilting encountered, and this direction we term the direction of free swing.

A simple form of Gyro-Compass pointing therefore in a direction, say, N.W. on board a rolling ship, has a force applied to it tending to turn it so as to bring its direction of free swing into line with the roll.

Anschutz gets rid of the error by multiplying the number of his gyro wheels, and by constructing the instrument as symmetrical as possible.

In England the quadrantal error was first discovered and studied, I believe, by the Admiralty Compass Department.

In the year 1914 the Sperry Co. claimed to have effected a cure for the error by attaching the pendulous weight, not directly to the gyro casing, but through a pin arranged to move in a slot in the casing.

In order that the axis of suspension of the pendulum may remain vertical when the compass oscillates with the rolling of the ship, a small auxiliary gyro was employed to stabilize the pin connection between the pendulum and the gyro casing.

We therefore see in these applications of Anschutz and Sperry two general ideas.

In the first case the idea is to make everything symmetrical, like a ball, so that there is no stabilized or free swing direction to the wheel, and therefore no tendency to turn; while in the second case a method is provided to prevent the point of application of the pendulum weight from moving, and acting as a crank, and by keeping the pendulum weight always vertical in the N.W. direction to destroy its power of turning the compass.

In the "Brown" Compass the quadrantal error is eliminated by making the weight operate completely out of phase with the roll—that is, at 90° displacement.

If a Gyro-Compass is worked by a weight which tends to precess

the wheel in phase with the roll, then there must be a quadrantal error, but there will be no error if it is forced to operate completely out of phase with it.

When I describe the "Brown" Compass I will explain how this is carried out.

It is also essential, as Anschutz has remarked, that the real moments of inertia shall be the same in all directions of the movable system of the compass—that is to say, the moving system should be in dynamic balance, as it is termed.

If a child's hoop is suspended by a string and is swinging in one direction, the hoop tends to set itself lengthwise to the direction of the swing. On the other hand, if an exactly similar hoop is placed

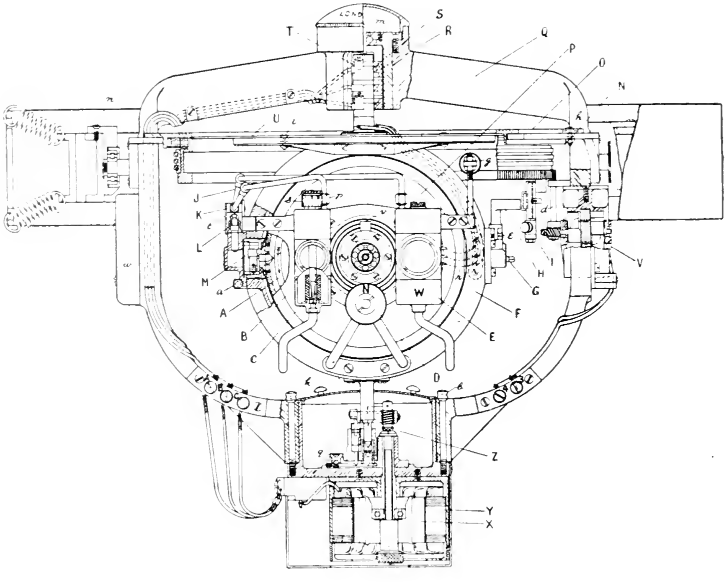

FIG. 7.

over but at right angles to the first, and suspended as before, then on swinging the hoops there will be no tendency for them to turn, as they are now in dynamic balance.

It is for this reason that the mass distribution of the moving system of the Gyro-Compass should be in dynamic balance, and to carry this out adjustable weights are fitted, usually in the direction of the spindle of the wheel, to counteract the weight of the supporting ring in the gyro casing, and thus there is no tendency for the compass to turn, due to this cause, when under the action of rolling.

The "Brown" Gyro-Compass is shown diagrammatically in Fig. 7.

A is the gyro wheel in its casing B. This case is carried on knife edges M in the vertical ring F, and is thus free to tilt under the action of the rotation of the earth.

The vertical ring turns in azimuth on a frictionless mounting, consisting of an oil pump at the bottom of a ball-bearing *m* at the top.

XY is the three-phase motor that drives the oil pump.

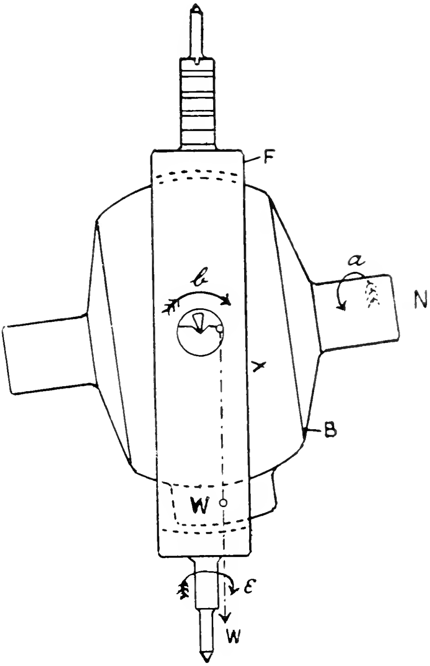

FIG. 8.—GYROSTAT.

The gyro wheel is the rotor of a three-phase motor, and current is led into the moving system through the three sets of iron contact rings R and S. These rings do not touch, but the outer set are hollow, and mercury fills the space between them, so that there is little friction.

The vertical ring is dynamically balanced by two projecting weights D.

Q is the pendulous mounting, supported by gymbal rings and by the outer row of springs to take up shock.

The gyro wheel runs at 15,000 revolution per minute, and thus acts as a powerful blower, giving an air-pressure equal to some 3 inches of water.

Fixed to the vertical ring, but connected through the hollow bearing *m* to the inside of the case, is the air jet L. This jet blows into the two halves of the air box K, and thence through the pipes J. The air-pressure is thus transmitted to the oil in the two sets of bottles C and D.

H is another air jet similarly mounted and employed to act upon a pair of contact-making vanes I.

The contacts I, through the agency of the controller which is fixed on the switch-board, is to work the repeaters and the step-by-step motor V. This motor forces round the follow-up ring N to keep the contact-making vanes I always opposite the air jet, and in doing this all the repeaters on the ship follow suit.

U is the compass card fixed to the upper portion of the vertical ring, and O is the lubber line support.

By removing the four screws marked *n* the Gyro-Compass can be completely removed from the gymbal rings.

The instrument thus removed is shown in Fig. 5.

To explain the action of the oil bottles I have introduced the Figs. 9 and 10.

Fig. 7 illustrates the simplest form of compass, in which the wheel and case B are controlled by the pendulous weight W.

When the case tilts as shown, W is moved to one side of the vertical support, and the weight tries to bring the case again to the horizontal.

Suppose the wheel revolves in the direction of arrow *a*, the righting torque is in the direction of arrow *b*, then the wheel and case will turn in azimuth in the direction of the arrow *e*.

Such a compass would have a quadrantal error, because the weight W would produce stresses in phase with the roll.

Fig. 9 illustrates the method of control of the "Brown" compass. When the case B is horizontal, the bottles EE are half full of oil, and the air jet L is blowing equally into the two halves of the air box K; but when the case tilts as shown, then the air pressure blows into one side of the box more than the other, and in such a direction as to force the oil from the lower bottle into the one raised. There is, therefore, a considerable righting torque indicated by the weight of the oil W trying to restore the case back again to the horizontal.

When the pendulum swings under the action of the rolls of the ship, the air jet L moves from one side to the other side of the air box in tune with the roll, blowing the oil periodically from one bottle to the other. At the middle of the swing of the pendulum the air jet is at the middle of the air box, and there is no difference of air pressure, and therefore no movement of the oil; and when the swing is at the end of its path and not moving, the air jet is at one side of

the air box and producing the maximum movement in the oil. It will therefore be seen that the movement of the pendulum and the movement of the oil are out of phase with each other. It is for this

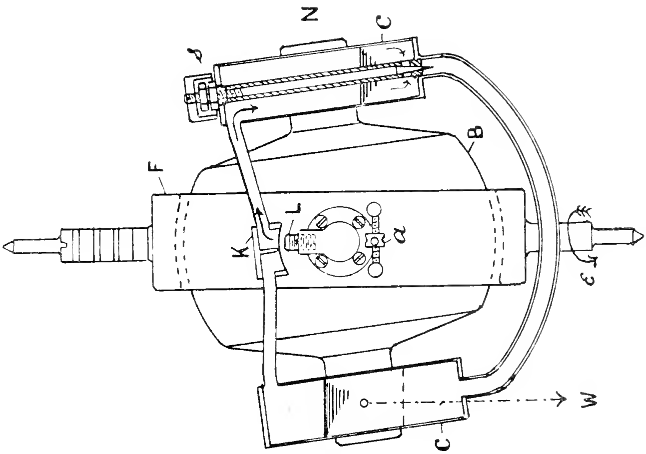

Fig. 10.

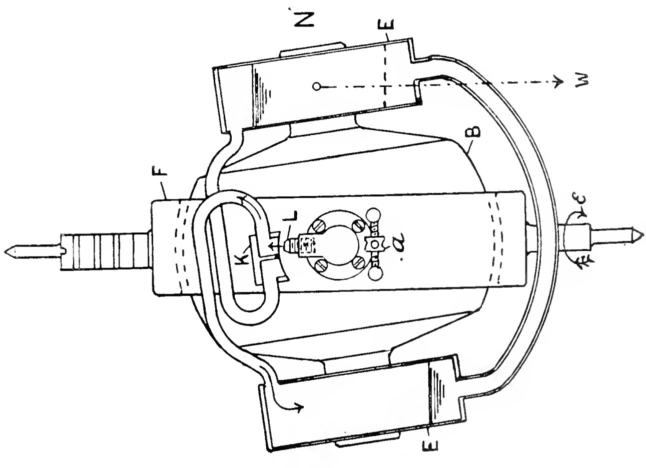

Fig. 9.

reason, given good dynamic balance, that there is no quadrantal error whatsoever with this method of control.

Fig. 10 illustrates the method of damping the compass. Fixed to

the same air box K are the two damping bottles CC, smaller than EE, but the air here acts in the opposite direction to that in the left-hand side of the figure.

In one of these damping bottles is the adjustable needle valve, and this valve has a constricted passage, and thus the flow of oil from one bottle to the other is suitably retarded.

As regards the accuracy of the compass, I may mention that one on board a flagship in the North Sea during the War was observed with particular care, especially during very heavy weather, and it was reported that it was never more than $1\frac{1}{2}^\circ$ from the true north position during the whole of the tests.

Trials on a commercial ship have demonstrated the fact that the employment of a Gyro-Compass resulted in the ship steaming every day over 3 per cent greater mileage: in other words, 1 day's steaming in 30 would be saved, resulting in a proportionate saving in coal and all other expenses.

I come now to a most important application of the Gyro-Compass—namely, its employment as a gun director.

The use of the gyrostat in the Whitehead torpedo has revolutionized naval strategy, and I believe the use of the same instrument in the form of a Gyro-Compass gun-director may possibly produce profound changes in gunnery practice in the future.

Modern naval warfare is entirely different to that of the past in the fact that the rival fleets come into action when separated by many miles. The guns have therefore to be worked and fired while the distant targets are invisible to the gunners.

The guns have to be directed by observers in an elevated position, these observers communicating the distance of the target and its direction in space.

The direction in space must be supplied by a Gyro-Compass on board the ship, and it is essential that the compass for this purpose be of extreme accuracy.

Once the guns are properly trained they may be joined up and be controlled by the Gyro-Compass, and for this purpose the turrets would be designed to act as huge repeaters, keeping the guns pointing on the target, changing only on receiving new directions from the observer.

The compass would hold the guns pointing on the distant target quite independent of the movements of the ship, which may at the time be steaming at full speed and manœuvring. Such movements are a great protection to ships against submarine and aerial attack.

It has been suggested that the day of the great battleship is over, but I am doubtful of this, as I understand that ships can be made proof against ordinary submarine attack by means of blisters filled with oil, as in our Navy, or by coal dust, as used by the Germans.

Working the guns with the ships at full speed, as I have just stated, will be an additional protection, while submarine craft will be

more dangerous operating against fixed objects, such as harbour defences, etc., in which case they could be detected from the shore by submarine listening devices, such as my liquid microphone.

I have now come to the end of my Discourse, but before closing I should like to say that I think a great deal of credit is due to Anschutz for the courage he displayed in being the first to attempt a Gyro-Compass, knowing, as he did, the extremely feeble force that is likely to result from the earth's rotation, and in the fact that the instrument must be carried on a rolling, pitching, plunging vessel. With us who follow it is a question over again of Columbus and the egg.

For myself, if I had known at the commencement of my acquaintance with the Gyro-Compass, some five years ago, all the difficulties that had to be encountered, I should I think have abandoned the pursuit. It was a case of "where angels fear to tread."

[S. G. B.]

GENERAL MONTHLY MEETING,

Monday, February 2, 1920.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

William Henry Bailey, M.D.
Mrs. Philip Champion de Crespigny,
James Campbell Dewar, C.E.
Mrs. Jessie M. Dreschfield,
G. Bramwell Ehrenborg,
William Rushton Parker, M.A. M.D.

were elected Members of the Royal Institution.

The Chairman reported that the following Letters had been received from Honorary Members elected at the General Meeting on December 1, 1919:—

THE ROCKEFELLER INSTITUTE FOR
MEDICAL RESEARCH,
66TH STREET AND AVENUE A,
NEW YORK.
December 23, 1919.

General E. H. Hills, C.M.G. F.R.S.

DEAR SIR,

Permit me to acknowledge the receipt of the Diploma conferring upon me honorary membership of the Royal Institution of Great Britain.

I beg you to convey to the Board of Managers and the Members of the Royal Institution, the expression of my deep appreciation and sincere gratitude for the high honour bestowed upon me.

I have the honour to be, Sir,
Your obedient Servant,
JACQUES LOEB.

FACULTÉ DES SCIENCES,
UNIVERSITÉ DE TOULOUSE,
Decembre 11, 1919.

MONSIEUR LE PRÉSIDENT
DE L'INSTITUTION ROYALE DE LA GRANDE BRETAGNE,

J'ai été extrement honoré de la décision par laquelle l'Institution Royale de la Grande Bretagne m'a designé comme Membre Honoraire. C'est

pour moi un grand honneur, que j'apprécie hautement, d'avoir été distingué par cette Compagnie qui a rendu de si grands services au developpement des Sciences et qui compte dans son sein si grand nombre de savants illustres.

Je vous prie, Monsieur le Président, de transmettre à l'Institution Royale mes meilleurs remerciements, et d'agréer l'expression de mes sentiments les plus devoués.

PAUL SABATIER,

Doyen de la Faculté des Sciences de l'Université de Toulouse ; Membre de l'Académie des Sciences de Paris ; F.R.S.

LICK OBSERVATORY,
UNIVERSITY OF CALIFORNIA,
MOUNT HAMILTON,
January 5, 1920.

DEAR GENERAL HILLS,

From the letter of Mr. Young, Assistant Secretary, dated December 4, I was mightily pleased to learn of my election as an Honorary Member of the Royal Institution. I have been privileged to attend two public lectures and the after meetings in the Royal Institution, and I have admired unreservedly its policies and purposes. The Royal Institution has been singularly effective in research; and I am in full sympathy with the idea that whatever in Science is worth discovering and preserving should be given to the people; for example, through the medium of lectures and otherwise.

I have received the Diploma of Membership.

Will you kindly express to the Board of Managers my acknowledgment of the honor and pleasure which they have given me?

Let us hope that the year 1920 will witness the solution of many important world and national problems now pending.

Yours sincerely,

W. W. CAMPBELL.

General Edmond H. Hills, D.Sc. F.R.S. etc.

INSTITUT PASTEUR,
25 RUE DUTOT, PARIS.
Decembre 6, 1919.

MONSIEUR LE SECRÉTAIRE,

Je vous accuse reception du diplôme de Membre Honoraire de la Royal Institution of Great Britain. Je suis très touché de l'honneur qui m'est fait par cette illustre Compagnie et je vous en exprime ma profonde gratitude. Je vous serai très obligé d'en faire part aux Membres du Conseil et de recevoir pour vous-même, l'assurance de ma haute considération.

DR. ROUX.

A.M., General Edmond H. Hills,
Secrétaire de la Royal Institution of Great Britain.

The Special Thanks of the Members were returned to Mr. A. B. Bence Jones for his Gift of Copies of Abstracts from Scientific Journals of Faraday's Friday Evening Discourses, 1826–1862, used by Dr. A. B. Bence Jones in his "Life of Faraday."

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, February 6, 1920.

HENRY E. ARMSTRONG, LL.D. F.R.S., Vice-President, in the Chair.

PROFESSOR SIR WALTER RALEIGH, M.A.

Landor and the Classic Manner.

[NO ABSTRACT.]

WEEKLY EVENING MEETING.

Friday, February 13, 1920.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

Professor W. M. Bayliss, M.A. D.Sc. F.R.S.

The Volume of the Blood and its Significance.

The system of vessels in which the blood is contained must be conceived of as a *closed* system. But the walls are distensible and elastic: they can therefore stretch and collapse to accommodate varying amounts of liquid. This is possible, however, only to a limited extent. Although the veins have thinner walls than the arteries, and appear to be less supported by surrounding structures than are the capillaries, it is remarkable that they oppose a greater resistance to a bursting pressure than do the arteries. Veins, moreover, have a muscular coat which is in a more or less contracted state during life. Hence the introduction of more fluid into the system must encounter a certain resistance and raise the internal pressure, unless the muscular coat actively relaxes to accommodate the fluid introduced.

This closed system contains, under normal conditions, about four litres of blood in man. It consists, as is generally known, of the heart, of branching tubes (arteries), leading from the heart to the tissues, where they break up into a network of much finer tubes, the capillaries, which unite again to form veins, and so lead the blood back to the heart. Consider the distribution of the blood at the time when the heart is at rest. The amount present in each part, including the heart itself, is obviously in proportion to the capacity of each part.

The heart, however, works as a pump. The way in which the blood is circulated was first clearly propounded by Harvey in 1616, although Leonardo da Vinci came very near to the discovery more than a century before. Harvey saw the blood sent out from the heart, propelled to the tissues in the arteries, and returned to the heart by the veins. The course of the blood from one to the other through the minute capillaries could not be seen until the invention of the microscope by Leeuwenhoek, who made use of it in 1686 to observe the blood traversing the capillaries in the tail of the tadpole.

The heart, then, when it contracts, drives out the blood which is contained in its cavities, or nearly the whole of it. This same

quantity must be returned by the veins, otherwise the blood would soon all be accumulated in the peripheral parts of the body. Further, the heart is capable of driving out the more blood the greater the quantity it contains when contraction begins. This is what has been called by Starling the "law of the heart." It depends on the fact that muscular fibres contract the more powerfully the greater the length to which they are stretched to begin with—within limits, of course.

We see, therefore, that the amount of blood driven through the organs of the body in a given time depends on the amount present in the heart at rest. Since this is a definite fraction of the whole blood, the irrigation, as we may call it, of the body is in proportion to the total quantity of blood available. The importance of sufficient irrigation is obvious. The blood conveys to the active cells the materials required for their work, and of these the most necessary is oxygen. If the supply is too meagre, the first few cells with which the blood meets exhaust it, and those beyond suffer from deprivation. Waste products are removed at the same time.

Although the part played by the volume of the circulating blood in relation to the capacity of the vascular system was realised by Carl Ludwig and his school, who made many experimental investigations on the subject, the matter came especially into prominence in connection with the explanation and treatment of the state known as "surgical shock," but which occurred with alarming frequency in men wounded in the late war. The name "wound-shock" is a more comprehensive name, although the use of the word "shock" is liable to give a misleading impression as to the rapidity of its onset, and to cause confusion with "shell-shock," another unsatisfactory name, but used to designate an affection of the nervous system of quite a different nature from that brought about by the wounds themselves. Wound-shock is not easily defined in such terms as to distinguish it clearly from other similar states, such as that due to loss of blood, but it may be said to be one of general collapse, ending in death if not combated in some way. It does not come on immediately after injury, but in the course of some two or three hours. It shows itself by pallor, coldness, sweating, vomiting, thirst, low blood-pressure, and the other symptoms which were early recognised as indicating a defective circulation.

But what is the actual cause of this collapse of the circulatory mechanism? It was soon realised, by those who examined cases of wound-shock, that it was not due to any failure of the heart itself, nor was the central nervous system involved, except indirectly in the later stages. On the other hand, much difficulty was found in distinguishing between this state, even when attended by very little loss of blood, and that resulting from great loss of blood unaccompanied by serious injury. The latter is obviously the result of the defective volume of blood and its consequences, since blood is known to have

left the body. But why do the former cases also appear to be suffering from the same condition, when scarcely any blood has actually been lost?

In the endeavour to find an explanation for this, we may call to mind the circumstance that blood may be effectively removed from circulation by being pooled away in some part or other of the vascular system, as, for example, by a great dilatation of this part. The amount which is available for propulsion by the heart to serve for continuous irrigation of the tissues is reduced as much as it would be if the blood held in the pool were actually lost to the outside. Such changes in the capacity of the peripheral blood-vessels play a large part in the regulation of the blood-pressure and the supply of blood to various organs. We may inquire whether anything of this kind happens after severe injuries.

The first step taken in the course of this enquiry was the discovery that some poisonous substance is produced in injured tissues. This, passing into the blood, is carried to all parts of the body. Sir Cuthbert Wallace, some years ago, had noticed that operations in which the cutting of large masses of tissue was involved were especially liable to be followed by shock. Quénu and others, during the war, were struck by the rapid benefit frequently ensuing from removal of the injured parts or even when they are tied off from connection with the rest of the blood-vessels, if such is possible. Cannon and myself found that we could produce the state of wound-shock in anæsthetised animals in the laboratory, and that it was due to a chemical agent, not to any effect on nerves. This being so, we see that we can replace the name of "wound-shock" by the more descriptive one of "traumatic toxæmia."

But can we form any conclusion as to the chemical nature of this toxic substance or as to the way in which it acts? It is evidently produced too quickly to be a result of bacterial infection, and, indeed, McNee was able to exclude this possibility quite definitely. Dale and Laidlaw, however, showed that there is a compound of known chemical structure, called "histamine," and produced without difficulty from a constituent of the nitrogenous cell structures, which is able to produce a state of the circulation like that present in wound-shock. It was found that the effect was not due to a dilatation of the arterial part of the system, as was known to be the case in the fall of blood-pressure brought about by vaso-motor reflexes. Here the similarity to traumatic toxæmia showed itself again, because it was known that arterial dilatation was not present in this state. Next, Dale and Richards, by a number of ingenious experiments, were able to localise the effect in the capillaries, which became widely dilated and thus capable of taking up the greater part of the blood in the body, leaving the heart nearly empty, with too meagre a supply to carry on the circulation with any degree of efficacy. It is to be admitted that we have not yet definite proof that it is histamine itself which is

responsible for the toxæmia of injury. But that the agent is something which acts in the same way is made clear by the observations that have been made on wounded men. The determinations of the volume of the blood in circulation, made by N. M. Keith, may be especially mentioned. Keith showed that, in severe cases, it may be reduced to little more than half the normal amount, although scarcely any has actually been lost by hæmorrhage. The method used was that of introducing into a vein a known quantity of an innocuous dye which does not pass through the walls of the blood-vessels, and, after a short interval, taking a sample of the blood and finding how much the dye has been diluted.

If the toxæmia is severe, a second property of the poison shows itself. This is an effect on the walls of the capillaries such that they allow the liquid part of the blood to escape by filtration. In this way the volume of the blood is still further reduced.

The treatment, in principal, is obvious. Restore the blood-volume. It would appear that when blood has been lost it ought to be replaced by blood. The case of traumatic toxæmia is not so clear at once, because blood has not been actually lost, and it should be possible to keep up an effective circulation by some other liquid until the poison is got rid of and the pooled blood returned to circulation. In fact, as experience increased, it was realised that the important matter is to maintain the volume in circulation, whether by blood or other solution. An innocuous fluid seemed to serve practically as well as blood, and had the advantage of being always at hand and in as large a quantity as required.

As to the properties of such a solution, it was soon found that a simple saline solution is very rapidly lost from the circulation and is useless. It is necessary to add to it some colloid with an osmotic pressure, such as gelatin or gum acacia. The colloid does not pass through the walls of the blood-vessels, and its osmotic pressure causes an attraction of water to balance that lost by filtration. Thus, although the slow circulation incidental to a small volume of blood is inadequate, this very quantity, if diluted to normal volume, is able to serve effectively. Comparing the oxygen carried by red corpuscles to railway passengers, it will be realised that if we have a limited number of trains, we can carry more passengers in a given time if the velocity of the trains is increased. Animal experiments made by Gasser showed that this is actually the case with the blood. After a loss of blood the injection of gum-saline might even raise the supply of blood-corpuscles to a level beyond what it was before the hæmorrhage.

The general conclusion is that the volume of the liquid in circulation must be kept up to its normal value, whatever this liquid may be. Of course, the number of red corpuscles cannot be allowed to fall below some particular value, and it has been found that about one-quarter of the normal quantity is the lowest compatible with life.

If they fall below this, moreover, there is no production of new corpuscles.

In the later stages of the war gum-saline was largely used in the British, American, and French Armies, and is reported to have saved many lives. Unfortunately, if too long a time is allowed to elapse before treatment, nothing avails, not even transfusion of blood. Hence the importance of the early use of intravenous injection, and also of removal of the injured tissue by operation. As the war progressed, these procedures were, therefore, pushed more and more forward to the battle area, and with more and more favourable results.

[W. M. B.]

WEEKLY EVENING MEETING,

Friday, February 20, 1920.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

EDWARD J. RUSSELL, D.Sc. F.R.S.

British Crop Production.

CROP production in Britain is carried on in the hope of gain, and thus differs fundamentally from gardening, which is commonly practised without regard to profit and loss accounts. Many poets from times of old down to our own days have sung of the pleasures to be derived from gardening. But only once in the history of literature have the pleasures of farming been sung, and that was nearly 2000 years ago.

> "Ah! too fortunate the husbandmen, did they but know it, on whom, far from the clash of arms, earth their most just mistress lavishes from the soil a plenteous subsistence."
>
> *Georgics*, *Bk. II.*, 1., 458 *et seq.*

"Did they but know it!" Even then there seem to have been worries!

This seeking for profit imposes an important condition on British agriculture: maximum production must be secured at the minimum of cost. This condition is best fulfilled by utilising to the full all the natural advantages and obviating as far as possible all the natural disadvantages of the farm: in other words, by growing crops specially adapted to the local conditions, and avoiding any not particularly well suited to them.

From the scientific point of view the problem thus becomes a study in adaptation, and we shall find a considerable interplay of factors, inasmuch as both natural conditions and crop can be somewhat altered so as the better to suit each other.

It is not my province to discuss the methods by which plant breeders alter plants; it is sufficient to know that this can be done within limits which no one would yet attempt to define. The natural conditions are determined broadly by climate and by soil. The climate may be regarded as uncontrollable—

"What can't be cured must be endured."

The scheme of crop production must therefore be adapted to the climate, and especially to the rainfall.

The rainfall map shows that the eastern half of England is on the whole drier than the western half. In agricultural experience wheat flourishes best in dry conditions and grass in wet conditions; the vegetation maps show that wheat tends to be grown in the eastern and grass in the western part. The strict relationship is that seed production is appropriate to the drier, and leaf production to the wetter districts.

The great soil belts of England south of the Trent run in a south-westerly direction; north of the Trent, however, they run north and south. A heavy soil, like a wet climate, favours grass production; a light soil, like a dry climate, is suitable for arable crops. The great influence of climate is modified, but is not overridden, by the soil factor.

The arable farmer grows three kinds of crops: corn, clover or seeds hay, and fodder crops for his animals, or potatoes for human beings. The same general principles underlie all, and as corn crops are of the most general interest (though not necessarily of the greatest importance), they will serve to illustrate all the points it is necessary to bring out. We have seen that wheat is cultivated more in the eastern than in the western portion of the country. The figures for consumption and production are as follows:—

Millions of Tons per Annum.

	Consumption in United Kingdom	Production in England and Wales			Production in United Kingdom		
		Before War 1914	1918	1919	Before War 1914	1918	1919
Wheat .	7·40	1·6	2·3	1·8	1·7	2·6	2·0
Barley .	1·96	1·2	1·2	1·1	1·6	1·5	1·3
Oats . .	4·30	1·4	2·0	1·6	3·0	4·5	4·2

During the War very serious attention was paid to the problem of reducing the gap between consumption and production. A working solution was found by lowering the milling standard, retaining more of the offal, and introducing other cereals and potatoes; a very considerable proportion of the resulting bread was thus produced at home. But the War bread did not commend itself, and disappeared soon after the Armistice; since then the consumption of wheat has gone up, and the divergence between consumption and production has again become marked. There is no hope of reducing consumption; we must therefore increase production. There are two ways in which additional production may be obtained—by increasing the yield per acre, and by increasing the number of acres devoted to the crop.

The yield per acre is shown in the following table :—

MEASURED BUSHELS PER ACRE.*

	(1908–1917) Average Yield per Acre		A Good Farmer expects	Highest Recorded Yield
	England & Wales	Scotland		
Wheat . .	31·0	39·9	40 to 50	96
Barley . .	31·9	35·4	40 to 60	80
Oats . .	39·3	38·9	60 to 80	121

The average results include bad farmers and bad seasons ; the good farmer expects to do considerably better, but he has many things in his favour—superior knowledge, greater command of capital and possession of good land : he will therefore always stand above the average. Even his results can be improved ; the highest recorded yields show what can be done with present varieties and present methods under exceptionally favourable circumstances. The figures give the measure of the scientific problem, which is to discover what changes would be necessary in order to bridge the enormous gap between the average and the best. In three directions progress is possible : we may modify the plant and the soil, or we may mitigate the effects of unfavourable climate.

Before the soil can be brought into cultivation at all it is necessary to carry out certain major operations, such as draining, enclosing, which have to be maintained in full order. These lie outside our present discussion ; we must assume that they are properly carried out, which is by no means always the case. Given adequate drainage, soil conditions are profoundly modified by cultivation, which has developed into a fine art in England and Scotland, and is indeed far better practised here than in most other countries. But it is an art and not yet a science ; the husbandman achieves the results,

* Unfortunately the terms "bushel" and "quarter" (8 bushels), lack definiteness, being used officially in three different senses, and unofficially in several others also. The following are some of the definitions of a bushel :—

	Official Statistics. A definite volume having the following average weight	Corn Returns Act. Volume occupied by following weight	Grain Prices Order. Volume occupied by following weight	Frequent Practice. Volume occupied by following weight
	lb.	lb.	lb.	lb.
Wheat . . .	61·9	60	63	63
Barley . . .	53·7	50	55	56
Oats	39·3	39	42	42

but no one can yet state in exact terms precisely what has happened. A beginning has been made, and a laboratory for the study of soil physics has been instituted at Rothamsted, and placed under Mr. B. A. Keen, where we hope gradually to develop a science of cultivation. For the present cultivation remains an art, and, further, it is essentially a modern art. The mediæval implements, as shown in the Tiberius MS. (11th Century) and the Louterell Psalter (14th century), were crude and left the ground in an exceedingly rough condition. Great advances were made throughout the 19th century. Robert Ransome, of Ipswich, took out his first patent in 1785 to improve the plough; he was followed in 1812 by Howard, of Bedford, and later by Crosskill, Marshall, Rushton, Fowler and others, who have made the name of British implement makers famous throughout the world. Given time and sufficient labour the good British farmer using modern implements can accomplish wonders in the way of cultivation.

Unfortunately neither time nor labour is always available. Ploughing is possible only under certain weather conditions, and there are many days in our winters when it cannot be carried out. Unless therefore a large staff of men and horses is kept, the work often cannot be done in time to allow of sowing under the best conditions.

The early days of the life of a plant play almost as important a part in its subsequent history as they do in the case of a child. Illustrations are only too numerous of the adverse effect of being just too late for good soil conditions. One from our own fields is as follows:—

Work Completed.	Seed Sown	Yield of Wheat 1916. Bushels per Acre.
Just in time	Nov. 24, 1915	26·8
Just too late	Feb. 17, 1916	19·3

The farm-horse will not be speeded up, but maintains an even pace of $2\frac{1}{2}$ miles per hour. According to the old ploughman's song still surviving in the village an acre a-day is the proper rate:—

"We've all ploughed an acre, I'll swear and I'll vow,
For we're all jolly fellows that follow the plough."

But under modern conditions it is impossible to get more than three-quarters of an acre a-day ploughed on heavy land, and the scarcity of teams threatened to bring arable husbandry into a hopeless impasse. Fortunately for agriculture, the internal combustion engine appeared on the farm in the shape of the tractor at a critical moment, and has brought the promise of a way out. The tractor

came from America, where the problem of farm labour had long been serious; we are still having to use American tractors, although English makes are now appearing. The English plough makers successfully coped with the engineering problems, and brought out an effective type of plough capable of doing excellent work.

The tractor has two important advantages over the horse.

First of all, it works more quickly. Its pace is $3\frac{1}{2}$ miles per hour instead of $2\frac{1}{2}$ miles. It turns three furrows at the time instead of one only; on our land it ploughs an acre in four hours, instead of taking nearly a day and a half as required by horses. There is no limit to the work it can do—even an acre an hour is no wild dream, but may yet be accomplished. It therefore enables the farmer to get well forward with his ploughing during the fine weather in late summer and early autumn, and thus to obtain the great advantages of a partial fallow and of freedom to sow at any desired time. On our own land our experience has been as follows:—

DATES OF COMPLETION OF SOWINGS OF WHEAT AND OATS.

Year	Wheat	Oats	
1916	Feb. 17	Oct. 16	Horses only
1917	March 16	Oct. 17	Horses only
1918	Jan. 26	Oct. 27	Horses only
1919	Nov. 26	Oct. 5	Tractor

Further, if the plough is correctly designed and properly used, the tractor does the work fully as well as horses—even the horse ploughman admits that. It therefore increases considerably the efficiency of the labourer, which, as we shall see later on, might advantageously be raised. The cost of working is apparently less, though it is difficult to decide this until one knows what the repairs bill will be. In our case the cost is:—

COST OF PLOUGHING PER ACRE, AUTUMN 1919.

	By Tractor s.	d.	By Horses s.	d.
Labour	7	7	10	2
Maintenance	—		22	6
Oil and Petrol	7	8	—	
Depreciation and repairs	6	3	—	
	21	6	32	8
Time taken	4 hours		$1\frac{1}{2}$ days	

The internal combustion engine is only just at the beginning of its career on the farm, and no one can yet foresee its developments

It is being used at present simply like a horse, and is attached to implements evolved to suit the horse. But it is not a horse; its proper purpose is to cause rotation, while it is being used to pull, and in some cases, indeed, this pull is reconverted into rotary motion.

The second great method of improving soil conditions is to add manures and fertilisers. Farmyard manure is more effective than any other single substance; it is likely to remain the most important manure, and if available in sufficient quantity it would generally meet the case. Realising its importance, Lord Elveden generously provided funds for extended investigations at Rothamsted into the conditions to be observed in making and storing it. This work is still going on, and is leading to some highly important developments.

Farmyard manure, however, is not available in sufficient quantities to meet all requirements. The chemist has long since come to the aid of the farmer: he has discovered the precise substances needed for the nutrition of the plant and has prepared them on a large scale. Like cultivation, this is largely a British development; it was here in London that the first artificial manure factory was established in 1842, and for many years the industry was centred in this country. The fertilisers now available are as follows:—

Nitrogenous . .	Nitrate of soda
	Nitrate of lime
	Sulphate of ammonia
	Cyanamide (nitrolim)
Phosphatic . .	Superphosphate
	Basic slag
	Mineral phosphate
	Guano
	Bones
Potassic . . .	Sulphate of potash
	Muriate of potash
	Kainit

Agricultural chemists have worked out the proper combinations for particular crops, and have obtained many striking results.

Without using any farmyard manure they have maintained and even increased the yield of corn crops, fodder crops and hay: and in the latter case there has been an increase not only in yield but also in feeding value per ton. In spite of seventy years' experience there is still much to be learned about the proper use of artificial fertilisers, and they may still bring about even fuller yields from the land.

The yields of corn crops can be increased by artificial fertilisers, but not indefinitely; the limit is set by the strength of the straw. As the plant becomes bigger and bigger so the strain on the straw increases, until finally when the plant is some 5 feet high it cannot stand up against the wind, but is blown down.

At present little is known about the strength of straw. It is a property inherent in the plant itself and differs in the different varieties.

It is affected by the season, being greater in some years than in others. It is affected also by soil conditions.

At present the strength of the straw is the wall against which the agricultural improver is pulled up. The problem can undoubtedly be solved, and the plant breeder and soil investigator between them may reasonably hope to find the solution.

Another great effect of artificial fertilisers which has not yet been fully exploited is to mitigate the ill effects of adverse climatic conditions. Phosphates help to counteract the harmful influence of cold wet weather; potassic fertilisers help the plant in dry conditions. The combination of a suitable variety with an appropriate scheme of manuring is capable of bringing about considerable improvement in crop production.

A demonstration with the oat crop on these lines was arranged last year in a wet moorland district, and the crops when seen in August were as follows:—

—	Estimated Crop	—
	Bushels	
Local variety, local treatment	27	Harvest late
,, ,, Phosphatic manuring	45–54	,, earlier
Special variety "Yielder," phosphatic manuring	54–66	,, earlier, stands up well

The potato crop is governed by the same general principles as corn crops. It furnishes more food per acre than any other crop, but it is much more expensive to produce, and therefore is grown chiefly in districts where the conditions are particularly well suited to it: the Fens, Lincolnshire, the plains of Lancashire, etc., the Lothians, though smaller quantities are grown in almost every part of the country. The production and consumption are as follows:—

POTATOES: ANNUAL PRODUCTION AND CONSUMPTION.
MILLIONS OF TONS.

Consumption	Production					
	In England and Wales			In United Kingdom		
	Pre-War 1914	1918	1919	Pre-War 1914	1918	1919
6·5	3·0	4·2	2·7	7·5	9·2	6·3
Millions of acres	0·46	0·63	0·48	1·20	1·51	1·22

We are thus self-supporting in the matter of potatoes. We do, however, import about half a million tons per annum of early and other potatoes; we also export seed potatoes and some for food, in all about one million tons per annum.

Fodder and hay crops play a more important part than cereals in the economy of the farm because they are the raw materials for a highly important part of the farmer's business, the production of meat, milk or butter. They are too bulky to transport in any quantity, and farmers use only as much as they themselves grow. The output of meat and dairy produce is therefore limited by the quantities of these crops at the farmer's disposal. The quantities produced just before the war and in 1918 were :—

PRODUCTION OF FODDER AND HAY CROPS.

	Yield per acre 1908–17		Acreage Millions of acres				Total produce Millions of tons	
	England and Wales	United Kingdom	England and Wales		United Kingdom			
			1914	1918	1914	1918	1914	1918
	tons	tons						
Swedes . . .	13·0	14·6	1·04	0·91	1·75	1·60	24·2	22·8
Mangolds . .	19·5	19·5	0·43	0·41	0·51	0·50	9·5	10·3
	cwts.	cwts.						
Hay (temporary) .	29·1	32·2	1·55	1·45	2·90	2·80	4·2	4·4
Permanent grass .	22·6	27·9	4·79	4·30	6·49	5·95	8·2	7·9

Like cereals and potatoes, these crops are greatly affected by artificial fertilisers, especially by phosphates, which increase not only the yield but also the feeding value per ton. This is strikingly shown in the case of swedes and turnips, which receive a large part of the superphosphate made in this country. Mangolds respond remarkably well to potassic fertilisers and to salt. There is much to be learned from a systematic study of the influence of artificial manures on the composition and feeding value of these crops under the varied conditions of this country.

A further reason for the important part played by these crops in the economy of the farm is that they profoundly affect the fertility of the soil. They do not remove from the soil all the fertilising constituents which must be added to secure maximum growth: some of these constituents are left behind in the soil to benefit the next crop—a rare instance of double effectiveness for which the farmer ought to be profoundly thankful. In the second place, even the fertilising constituents which are absorbed by the crop are not entirely retained by the animal: considerable quantities are excreted and pass into the manure, again to be added to the soil. There

is therefore the possibility of constant improvement of the soil; larger fodder crops enable more livestock to be kept, more livestock make more manure, and more manure gives still larger crops. It is sometimes argued that meat or milk production is in some way opposed to corn production, but on this method there is no antagonism: on the contrary each helps the other; the production of more meat is consistent with, and indeed involves, the production of more corn.

The simplest way of utilising animal excretions without loss is to allow the animals to consume the crop on the land where it grows, and this is frequently done excepting where the soil is so sticky as to become very unpleasant in wet weather. Sheep are the best animals for the purpose as they are easily penned in by light hurdles, these being moved as each portion of the field is cleared; this folding is a common occurrence on the chalky and sandy soils of the southern and eastern counties.

Bullocks are less tractable and cannot be enclosed by light hurdles; they are therefore generally kept in yards, roofed in if possible, but oftentimes open. Sufficient straw is added to provide them with bedding and to soak up the excretions. In this way the fertilising constituents of the straw as well as of the food are returned to the soil.

In the case of dairy cows the treatment is rather different; they have to be properly housed in quarters which are sometimes palatial, and for hygenic reasons they are allowed but little bedding. Their manure is removed once daily—sometimes oftener—the primary object being to get it away without contaminating the milk. The investigations already referred to for which Lord Elveden provides the funds are now being extended to the dairy farm to see how far it is possible to save the manure without prejudice to the purity of the milk.

In the old days when farmyard manure was the only manure, and the old type of implements alone were available, farmers had to arrange their crops on a definite plan in order to get through their work and permanently maintain the productiveness of the land. There thus grew up a system known as the rotation of crops, which contributed very largely to the agricultural developments of the 'sixties, and ultimately became a rigid rule of husbandry strictly enforced over large parts of the country. Modern cultivation implements and fertilisers justify much more latitude, however, and no good farmer ought to be restricted in his cropping, provided of course that he maintains the fertility of his land. It is sometimes a convenience on the dairy farm to grow the same crop year after year on the same land, and the Rothamsted experiments show that this can be done, excepting only in the case of clover. With this exception there is no more need to have a rotation of crops than to have a rotation of tenants in a house. It is essential, however, that the land be kept free from other competitors and from disease germs. Freedom

from competition means the exclusion of weeds. In the old days this had to be effected by periodical bare fallows. Nowadays a different course is possible; modern cultivation implements worked by a tractor allow great scope for the suppression of weeds. There is, however, one crop that must be grown periodically to ensure the best results—clover or a mixture of clover and grass. Clover affords valuable food for cattle during winter, and it also enriches the soil in highly valuable nitrogenous organic matter Much of this is the work of the plant itself and could equally well be done by grass: but the enrichment in nitrogen is the work of bacteria residing in the nodules in the clover roots and is unique among the phenomena of the farm.

Unfortunately clover, unlike other crops, cannot be grown frequently on the same land, and consequently the farmer is unable to make as much use of it as he would like. Investigators have for many years been trying to increase the effectiveness of the clover organism, but without result. Inoculation of the soil with virulent strains has been tried, but it was unsuccessful in this country, although results are claimed in the United States. The problem has recently been taken up at Rothamsted, and one reason found for the previous failure. The organism has several stages in its life-history, one of which is a period of rest; some conditions favour a long rest, others a shorter one, and Mr. H. G. Thornton is endeavouring to find out how to increase the activity of the organism in the soil and ensure that its work shall be done. Attention is being devoted also to the causes of failure of the crop. The clover crop furnishes some of the most important problems in arable farming before us.

In the meantime a working solution lies in growing an admixture of grasses with the clover; this reduces the risk of failure, while considerably benefiting both soil and farmer.

A typical arable district is thus a busy region in which both farmers and workers are kept constantly occupied. The crops claim attention all through the year, and particularly in summer, while in winter the animals need attention. Four or more men can be regularly employed per 100 acres. An organised village life has developed having distinctive characteristics of its own and presenting endless scope for the intelligent social worker.

Grass farming, on the other hand, stands out in sharp contrast with all this. The grass farmer puts his animals into the fields, and Nature does the rest; when they are fat he sells them to the butcher. It is essentially summer work, the winters are left free. As no man can long remain idle, there has been an extensive development of hunting and its attendant occupation, horsebreeding, in the English grass regions. While the grass farmer's life is not all idyllic joy, it is at any rate free from much of the worry and uncertainty of arable farming, and it brings in sufficient money to ensure a modest com-

petency; one can quite understand the reluctance of the farmer to quit this path of safety.

If one could accept the doctrine that a man could do what he liked with his land the grass farmer could be left alone and reckoned among Virgil's too happy husbandmen. But this doctrine is now somewhat out of court, and the needs of the community have also to be taken into account. From this point of view grass husbandry, in spite of its safeness for the individual farmer, is not as good for the community as arable farming, since it is less productive per acre of ground. This was realised before the war, and was vividly brought to the notice of farmers by Sir Thomas Middleton, who drew up the following table:—

Number of Persons who could be Supplied with Energy for One Year from the Products of 100 Acres of

Poor pasture converted into meat	2–4
Medium pasture ditto	12–14
Rich pasture ditto	25–50
Arable land producing corn and meat	100–110

The area of rich pasture is very restricted. An improvement can often be made in poor and medium pasture by the use of basic slag, by drainage and in other ways, but the results could probably never surpass those now obtained on rich pasture. None of them approach the results obtained on arable land.

During the War, therefore, the policy of the Food Production Department was to convert grass land into arable, and much was done; but now that the shadow of D.O.R.A. no longer lies over the land some of the arable is going back to grass. It is not that the farmer is trying to avoid work; he is impressed by the greater risk of arable farming,* and above all he desires to keep to the well-established principle that his system of husbandry must suit the local

* On our ordinary farm at Rothamsted (distinct from the experimental land), the expenditure on arable land is continuously increasing, while that on the grass land is much less. The figures are:—

—	1913-14	1917-18	1918-19
	£ s.	£ s.	£ s.
Wheat	5 7	10 14	14 0
Oats	6 4	9 7	14 5
Roots	17 10	20 18	36 0
Potatoes	21 1	37 11	46 0
Grass (Hay)	3 12	4 16	6 0
,, (Grazing)	2 15	2 4	3 0

About 40 per cent. of the expenditure on arable land goes in direct wage payments, but less than 15 per cent. of that on grass land.

conditions. This is strikingly shown by the following returns from a large number of farms :—

COLLECTED BY THE AGRICULTURAL COSTINGS COMMITTEE.

—	Income per Acre			Expenditure per Acre			Profit* per Acre			Capital per Acre		
	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.
England and Wales—												
Mixed farms . .	9	12	5	10	2	11	1	7	2	13	9	0
Dairy farms . .	14	17	6	13	18	5	1	7	4	15	7	0
Corn and sheep .	7	7	1	7	4	10	1	14	2	12	16	9
Large sheep farm .	1	4	3	0	17	6	0	8	5	1	7	10
All Scottish . .	5	10	9	4	15	10	1	4	11	7	7	9

* Including change in valuation.

The profit per acre from the large sheep farm is small in itself, but it is large in proportion to the capital and the expenditure, and given a sufficient acreage the farm is more lucrative than the more risky mixed or dairy farms. The risk of corn production can and probably will have to be met by some system of insurance or guarantee; but the need to conform to local conditions will always remain.

The problem therefore arises—can a system of husbandry be devised which suits the natural conditions as well as grass, and which is as productive of total wealth as arable crops? I believe this can be done. Grass is not the only crop adapted to moist conditions or heavy soils, and appropriate for the production of meat and milk. Many other leaf or root crops serve as well, some of which yield much more food per acre than does grass. Vetches, rape, mangolds, kale, marrow-stem kale, can all be used direct, and there are various mixtures of oats with peas, tares, vetches, etc., that can be fed green, made into hay or into silage as the farmer may wish. The use of these crops in the place of grass for the feeding of live-stock is known as the soiling system.

We are only just beginning to discover the combinations of crops best suited to particular conditions. An interesting experiment is in progress at the Harper Adams Agricultural College, which, however, should be repeated elsewhere. Each crop is governed by the same general laws as hold for cereals. In each case the yield and feeding value can both be increased by the proper use of artificial fertilisers, and there is the further possibility of great improvement by the plant breeder.

It is in this direction that I think British agriculture will develop in the future. The system is strictly in accordance with the laws of science, and therefore it needs a minimum amount of artificial support. It gives the farmer abundant scope for the production of livestock, which he has always regarded as his sheet anchor. It

gives the community an abundant production of food per acre, and, best of all, while retaining the best features of our present arable and grass systems, it allows of considerable further development.

I shall not venture any opinion as to how far we could go in feeding ourselves. The following table shows what we did before the War, and what, on our present technical knowledge, we could do now, assuming that the insurance problem of covering the extra risks of arable farming were solved, and assuming also a reasonable increase in the efficiency of labour :—

CONSUMPTION AND PRODUCTION OF HUMAN FOOD IN THE UNITED KINGDOM. MILLION TONS PER ANNUM.

—	Consumption (1909–13)	Home Production		
		Pre-war	1919 *	Estimated attainable
Wheat, barley and oats	13·4	6·5	7·0	10·0
Other cereals . .	3·5	—	—	—
Potatoes . . .	5·5	4·8	6·3	7·0
Dairy produce . .	5·2	4·7		5·0
Meat . . .	3·0	1·8		2·5

In this country we can certainly hope to find the solution of the insurance problem, and I hope and believe of the labour problem also. Our output per acre of the arable crops is distinctly above that of many other countries, though we no longer lead as we did in the 'sixties. Our output per man, however, is not particularly good and

* Mr. McCurdy gives the following details for 1919 (see *Times*, Feb. 18, 1920) :—

CONSUMPTION AND PRODUCTION OF FOOD IN THE UNITED KINGDOM, 1919.

Commodity	Estimated Total Consumption	Proportion of Home Grown and Imported Produce included	
		Home Grown	Imported
	Tons	Per cent.	Per cent.
Wheat	7,395,000	27	73
Barley	1,956,000	64	36
Oats	4,297,000	92	8
Beef and veal ..	995,000	66	34
Mutton and lamb ..	368,000	57	43
Bacon and hams ..	447,000	19	81
Butter	180,000	58	42
Cheese	145,000	30	70

NOTES.—*Cereals :* The quantities are given after deduction for seed, and in the case of wheat for tailings also. *Bacon :* The quantities given are for bacon as smoked or dried.

is open to considerable improvement. Those of us who know the agricultural labourer have the fullest faith that his sterling qualities will enable him to rise to the new levels of industrial capacity which the man of science and the engineer have opened out for British agriculture. There are anxious days ahead, but with wise and sympathetic treatment the difficulties can be solved and our future assured.

[E. J. R.]

WEEKLY EVENING MEETING,

Friday, February 27, 1920.

COLONEL E. H. HILLS, C.M.G. F.R.S., Secretary and Vice-President, in the Chair.

W. B. HARDY, F.R.S.

Problems of Lubrication.

IN lubrication a fluid or other body is used to decrease the friction between opposed solid faces. The lubricant may act in one of two ways. It may separate the faces by a layer thick enough to substitute its own internal friction, modified by the mechanical conditions in which it finds itself, for that of the solid faces; or it may be present as a film, too thin to develop its properties when in mass, which reacts with the substance of the solid faces to confer upon them new physical properties. In the latter case the solid faces continue to influence each other, not directly, but through the intermediation of the film of lubricant. There are indications that these two types of lubrication—the one in which the solid faces intervene only owing to their form, rate of movement, etc., and not by their chemical constitution; the other in which the chemical constitution is directly involved—are discontinuous states, in that the one cannot be changed gradually into the other by simply thinning the layer of lubricant. The change from the one to the other is probably abrupt.

It may by no means be asserted that resistance to relative motion is always least when the solid faces are floated completely apart; it would indeed probably be truer to say of the best lubricants, that friction is least when the "boundary conditions," to use Osborne Reynolds's phrase, are fully operative.

This address is concerned wholly with "boundary conditions," and we get directly to the heart of the problem by certain simple experiments. If a glass vessel, such as a bottle, be placed upon an inclined pane of glass at a certain angle, it slips smoothly down. The glass plate is an ordinary plate cleaned with a cloth. In the usual sense of the word the surface of the plate is not lubricated, the surface is "dry." The lower half of the plate is then wetted with water, and the bottle is now found to slip on the unwetted part and to be sharply pulled up by friction when it reaches the wetted part.

It is not sufficient, therefore, to interpose a liquid film between solid faces to get lubrication. Indeed, as the experiment proves, water increases the friction: it is an anti-lubricant for ordinary faces of glass.

Is then the quality of lubricant a property of a fluid? Does water fail to act merely because it does not possess that property to which the name "oiliness" is sometimes given? Another simple experiment supplies the answer. Instead of a glass plate let us use a plate of ebonite. The glass plate does not readily slip on this. The angle at which slipping occurs is steeper than when a glass plate is used. Now, when the lower half of the ebonite plate is wetted it is found that a glass bottle encounters relatively high friction on the unwetted part, but slips quite freely on the wetted part. Water, in short, is an admirable lubricant for glass on ebonite.

Here is another plate, picked up at random in the laboratory of the Royal Institution. Its composition is unknown. Tested in the same way, water has no detectable influence on the friction between glass and the surface of this plate.

It will be well to confess at once that these simple experiments raise questions which are as yet without an answer, and that much of what follows concerning them is merely tentative. They seem to establish two things, the first being the curious paradox that a film of fluid introduced between two surfaces does not always decrease friction; it may, indeed, very much increase it. The second, that the quality of "oiliness," the quality, that is, which enables a substance to act as a lubricant, seems to be not the property of a given fluid, but only of that fluid considered in reference to a particular surface.

It is necessary at this stage to clear away a possible explanation of the paradox. When two solid faces are separated by a thin film of fluid, capillary forces operate and in certain cases, at any rate, they resist slipping. They will so act, for instance, when the movement of the one face past the other increases the area of the free surface of the film. Water has a high surface tension: the capillary forces to which it gives rise are unusually large. Therefore it is pertinent to ask whether, when a layer of water diminishes the facility for the slipping of glass on glass, it is owing to capillary action. A qualitative answer is to be found in the fact that water does in some cases, as when glass is applied to ebonite, increase the facility for slipping; and Lord Rayleigh furnished the quantitative answer. He calculated the magnitude of the capillary effect and found it negligible compared to the actual friction of glass on glass wetted with water. An appeal to capillary forces of this type will not solve the paradox.

Some light is thrown upon it when we enquire into the state of the surface of glass whose friction is increased by water. Surfaces of glass "cleaned" in the ordinary way by rubbing with a glass-

cloth, or glass faces which have been simply exposed to the air, are in point of fact not clean; they are highly lubricated with a film of matter derived from the cloth or condensed from the atmosphere. This "grease" film is of invisible thinness. It is probably of the order of one $\mu\mu$ in thickness, that is to say, one millionth of a millimetre. It can be removed by soap and water, which in turn must be removed by a stream of water, and the plates dried in clean air out of contact with solids. The film re-forms quickly—very quickly in London air, and less quickly in the country. A "grease" film also creeps over a cleaned glass face from ordinary solids with which it may be in contact. Still, when due precautions are taken, and they are many, it is possible to get a glass face which seems to be really clean.

The first property of such faces is that their friction, one for the other, is very high; indeed it is impossible to make them slip past one another. One glass plate may be forced past another, but true slipping does not take place, they tear at the point or points of contact. It is easier in short to disrupt the actual substance of glass itself than to get the surfaces to slip over one another. Clean glass faces "seize" when they touch.

When chemical substances are tested as lubricants on clean glass faces a remarkable fact emerges—namely, that some are quite neutral in that they do not alter the resistance to slip in the least; such are water, alcohol, benzene, strong ammonia. Other substances have some lubricating action great or small. That is to say, they decrease the force needed to produce slipping: such are the alkalies, trimethylamine and tripropylamine, the fatty acids, e.g. acetic acid, and the paraffins. Those fluids which act as lubricants are not necessarily fluids of any considerable viscosity, indeed a high viscosity is compatible with the absence of any true lubricating action. Thus glycerine facilitates the slipping of clean glass on clean glass only when it is present in quantity sufficient to float the surfaces apart. On the other hand, acetic acid and tripropylamine, substances of low viscosity, are admirable lubricants.

No fluid amongst those tested has been found to raise the friction of clean glass faces. A fluid either is neutral or decreases friction to a greater or less extent. The property of increasing the friction of glass faces which neutral fluids, such as water, possess, is due not to their action on the glass, but to the fact that they interfere with the effect of the invisible grease film. Water on an ordinary glass face acts as an anti-lubricant; on really clean glass it is "neutral."

All solid faces, however, do not distinguish chemical substances into those which are "neutral" and those which possess lubricating properties. Nearly one hundred substances have been tested on burnished faces of bismuth, and in every case some decrease of friction was observed.

A comparison of the lubricating action of simple chemical sub-

stances on clean faces of glass, and of bismuth, would seem to show that the quality of oiliness is due to some reaction between the substance and the solid face. Much is still obscure, but certain facts seem to be capable of interpretation in no other way. Thus water and ethyl alcohol have no detectable lubricating action on clean glass, whilst both are moderate lubricants for clean bismuth.

The thickness of the layer needed to lubricate is astonishingly small. It is quite invisible, and probably only one or a very few molecules thick. To discuss this adequately would take too long, but the fact may be instanced by an experiment of great beauty. A tiny drop of, say, acetic acid or tripropylamine is placed near one corner of a plate of clean glass 6 cms. square; nothing detectable by the senses happens, the drop is there and that seems to be all. But the whole surface of the plate has in fact been changed fundamentally. It is now fully lubricated by an invisible film which has spread rapidly over it from the drop. The presence of this film may be detected by measuring the friction or by following the migration of two drops of fluid over the face of the plate. It will be found that the drops attract one another under conditions which point to the cause being the contractility of the invisible film.

This brings me to the second part of my subject—namely, the relation of lubricating power to chemical constitution.

In particular experiments with bismuth a slider having a curved surface was applied to a plain surface of metal, both surfaces being highly polished, and the force required to initiate movement was measured. This force measures what is usually called static friction as opposed to the kinetic friction when the surfaces are in relative motion. The static friction was found to be a function of the weight of the slider. Therefore, as a relative measure, the ratio of the weight of the slider to the weight needed to move the slider was used. The results appear in the following Table:—

Static Friction for clean faces ·5.

CHAIN COMPOUNDS.

ALCOHOLS.

	Static Friction.		Static Friction.
Methyl	·29	Isopropyl	·32
Ethyl	·32	Isobutyl	·30
Propyl	·34	Allyl	·29
Butyl	·30	Glycol	·30
Amyl	·27	Glycerol	·22
Octyl	·25	Penterythritol	·40
Cetyl	·17		

ACIDS.

	Static Friction.		Static Friction.
Formic	·45	Stearic	·15
Acetic	·40	Oleic	·10
Propionic	·31	Ricinolic	·02
Valeric	·28	α Lactic	·20
Carpryllic, fluid	·19	Glyceric	·22
,, frozen on plate	·05		
Acetone	·32	B.P. "Paraffin"	·20
Methyl ethyl ketone	·29	Solid paraffin, m.p. 39·5	·09
		,, ,, m.p. 46	·07
Ethyl acetate	·36	Carbon tetrachloride	·43
,, valerianate	·35	Chloroform	·30
Tristearin	·24		
Triolein	·14	Amylene	·26
Acetone di carboxylic di ethyl ester	·29	Octylene	·28
		Ethyl ether	·33
n. Hexane	·37		
n. Heptane	·346	Butyl xylene	·27
n. Octane	·32		

RING COMPOUNDS.

Benzene	·34	Thiophenol	·22
Ethyl benzene	·32	Benzylhydrosulphide	·23
Iodo benzene	·30		
Toluene	·28	Pyridine	·33
Xylene	·30	Piperidine	·32
p-Cymeme	·31		
		Naphthalene	·29
Phenol	·25	Anthracene	·26
Catechol	·39	β Naphthol	·38
Quinol	·40	Naphthoic acid	·39
m-Cresol	·26		
Benzyl alcohol	·31	Carvacrol	·23
		Thymol	·24
Benzoic acid	·38	Menthol	·26
Phthalic acid	·37	di Pentene	·31
Cinnamic acid	·27	Camphor	·24
Benzilic acid	·45	Active ethyl ester of Camphor oxime	·33
Salicylic acid	·41	Iso-Cholesterol	·27
Ethyl benzoate	·33		
o-Phthalic ester	·27		
Ethyl hydrocinnamate	·28		
Ethyl cinnamate	·32		

CYCLIC COMPOUNDS.

Cyclohexane	·31	Cyclohexanone	·35
Methyl cyclohexane	·30	1.2. Methyl cyclohexanone	·32
1.3. di Methyl cyclohexane	·29	1.3. ,, ,, ,,	·35
		1.4. ,, ,, ,,	·33
Cyclohexanol	·20		
1.2. Methyl Cyclohexanol	·28		
1.3. ,, ,,	·25		
Ammonia fortiss	·34	Castor oil	·03
Triethylamine	·30		
Tripropylamine	·26	Water	·33

It will be seen that static friction is a function of the molecular weight of the lubricant; and in a simple chemical series of chain compounds, such as fatty acids and alcohols or paraffins, a good lubricant will be found if one goes high enough in the series. But it is not a simple function. The friction, for instance, rises sharply in moving from $CHCl_3$ to CCl_4 and from phenol to catechol and quinol. The influence of molecular weight is over-shadowed by the influence of chemical constitution.

In some simple chemical series the relation appears to be a linear one. Examples are paraffins; the series benzene, naphthalene, anthracene.

The relation of lubricating qualities to viscosity broadly resembles that to molecular weight. In a simple chemical series lubrication and viscosity change in much the same way with molecular weight: but that there is no fundamental relation between viscosity and lubrication is shown by the following figures:—

	Viscosity at 20°.	Static Friction.
Carbon tetrachloride	·0096	·43
Chloroform	·0056	·30
Acetic acid	·0122	·40
Octyllic acid...	·0575	·19
Benzene	·0065	·39
Toluene...	·0058	·28
Benzyl alcohol	·0558	·31

Fluidity of the lubricant has no constant significance. The curves for acids, alcohols and paraffins show no break where, with increasing molecular weight, the lubricant becomes a solid at the temperature of observation. Compare also benzene, naphthalene and anthracene, menthone and menthol, thymol and carvacrol.

Perhaps the most unexpected result is the distinction between ring and chain compounds. The simple ring compounds benzene, naphthalene and anthracene show the linear relation to molecular weight, and the values are much the same as those for paraffins of the same molecular weight. The similarities, however, end here, for any change in the molecular structure produces opposite effects according as it takes place in a chain or ring. Thus a double bond decreases the lubricating action of a ring compound, but increases that of a chain compound. As examples, compare naphthoic acid with double-bonded oxygen, with naphthalene, menthone with menthol, cyclo-hexanone with cyclohexane, benzoic acid with benzene. As examples of double-bonded carbon, compare cinnamic ester with hydro-cinnamic ester, di-pentene, having two unsaturated carbon atoms, with menthol and cyclohexane. Also the more saturated cyclic compounds are better lubricants than the less saturated ring compounds..

When a ring or chain are joined, as in butylxylene, the result is a better lubricant than either.

The esters occupy a quite unexpected position. The simple aliphatic esters are much worse lubricants than their related acids and alcohols. The ring esters, on the contrary, are better lubricants than are their related acids (e.g. ethyl benzoate and benzoic acid).

Perhaps the most interesting substances are the hydroxy acids with OH and COOH groups. This conjunction produces a remarkable increase in the lubricating power of a chain compound (lactic acid and ricinolic acid), and almost destroys lubricating action in the case of the ring compounds (salicyclic and benzylic acids).

In the ring compounds the replacement of hydrogen decreases lubricating power in the case of N, : O, or ·COOH, and increases it in the case of other groups in the order $C_2H_5 \angle CH \angle OH$.

The effect of a second group of the same or of a different kind is to decrease the effect of the first. Compare, for instance, toluene with xylene; catechol, quinol, and cresol with phenol; and methyl cyclohexanol with cyclohexanol. The simpler the group the more effective it is. Compare cymene with toluene or xylene, and benzyl alcohol with phenol.

When the atoms are disposed with complete symmetry about a carbon atom, the result is a very bad lubricant, as we see in carbon tetrachloride and the alcohol penterythritol $C(CH_2OH)_4$.

It will be noticed that no ring compound is a good lubricant. Even cholesterol, with the molecular weight 366, is no exception.

The group SH acts much as OH, thiophenol C_6H_5SH and benzylhydrosulphide $C_6H_5.CH_2SH$ resembling phenol and benzyl alcohol respectively.

Concerning one matter, and that the most fundamental, some conclusion must be come to, even though it be upset later. What is friction due to? The "Encyclopædia Britannica" is in no doubt as to this. Friction, it says, is due to inequalities of the surface. This conclusion cannot, I think, be accepted. Why, if it be true, should clean burnished faces of glass or bismuth refuse to slide over one another? It does not even accord with such simple facts as we now know. For instance, the friction of an optical face of glass was found to be the same as that of ordinary plate glass within the limit of accuracy aimed at. And both the optical face and ordinary plate were found to give higher values than ground glass.

The subject cannot be fully discussed here, but I think we may conclude with some confidence that the friction both of lubricated and of clean faces is due to true cohesion—to the force, that is, which binds together the molecules of a solid or of a fluid. If there were no seizing, there would be no friction. The function of the lubricant is to diminish the capacity for seizing by saturating more or less completely the surface forces of the solid. In some cases it seems to abolish it completely so that static friction vanishes.

The subject of lubrication is of interest to the engineer, but it is of perhaps more interest to the physicist, for it offers a means of

exploring the most difficult region of the physics of boundary zones—namely, the surface energy of solids. It will, for instance, I believe, enable us to prove that the simplest chemical change at the surface of a metal takes place only when the surface energy is decreased thereby. The film of oxide or sulphide which forms on copper acts as a very effective lubricant, and it acts also like a grease film in preventing water from wetting the surface; and from both of these facts we may conclude that the presence of the film lowers the surface energy of the metal.

[W. B. H.]

GENERAL MONTHLY MEETING.

Monday, March 1, 1920.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir William Henry Bragg, K.B.E. D.Sc. F.R.S.
W. Carter, M.A.
Mrs. Stanton Coit,
Major-Gen. T. M. Corker, C.B. M.A. M.D. LL.D.
J. F. Crowley, B.Sc.
Captain Sir Charles Cust, Bt., R.N. G.C.V.O. C.B. C.M.G.
Charles F. de Ganahl,
Kenneth Gray,
Miss A. N. Kershaw, B.Sc.
Miss M. H. Kinnear,
Miss Evelyn A. McGhee,
Brig.-General Ernest Makins, C.B. D.S.O.
Captain Horace George Mason, R.G.A. B.Sc.
Clayton Conyers Morrell, M.D.
Lady Rayleigh,
William Bristow Saville,
Ker George Russell Vaizey,
Miss R. V. Wagner,
Frederick Womack,
Lieut.-Col. Vincent Wright, D.Sc. F.R.G.S.

were elected Members.

The Chairman reported that the following Letters had been received from Honorary Members elected at the General Meeting on December 1, 1919 :—

THROOP COLLEGE OF TECHNOLOGY,
PASADENA, CALIFORNIA.
January 19, 1920.

SIR,

I wish hereby to acknowledge the receipt of the diploma of the Royal Institution, and to express to the Officers of that distinguished body my keen appreciation of the honor which has been done me by election to its Honorary Membership Such recognition from British Scientists I regard as the highest honor which the Scientific World has to offer.

Permit me to remain,
Your very obedient Servant,
R. A. MILLIKAN.

DEPARTMENT OF PHYSICS,
CLARK UNIVERSITY, WORCESTER, MASS.
February 17, 1920.

MY DEAR SIR,

Allow me to express my very great appreciation of my election as Honorary Member of the Royal Institution.

I am in receipt of the Diploma, together with the handbook of the Institution, which you were so kind as to send me.

Very sincerely yours,
ARTHUR GORDON WEBSTER.

The Special Thanks of the Members were returned to Mr. Sidney G. Brown, M.R.I., for his Donation of £12 to the General Fund of the Institution.

The Chairman announced the decease, on February 19, 1920, of Dr. James Emerson Reynolds, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and Chemical Science, by the death of Dr. James Emerson Reynolds, M.D. D.Sc.(Dub.) F.R.S. F.C.S., Past-President of the Chemical Society, late Professor of Chemistry, Trinity College, University of Dublin, and a late Manager of the Royal Institution.

Dr. Emerson Reynolds was successively Keeper of the Minerals at the National Museum of Dublin, Professor of Analytical Chemistry at the Dublin Royal Society, and Professor of Chemistry to the Royal College of Surgeons in Ireland.

In 1869 he succeeded in the Isolation of Sulphur Urea, a most important step in Organic Chemistry, owing to the large variety of new and interesting compounds which resulted from its use as an agent of research. In 1871 he discovered a new group of Colloid Bodies containing Mercury.

After occupying the Chair of Chemistry in Trinity College, Dublin, for twenty-eight years, he took up his residence in London, and was a Member of the Royal Institution, of which he was an earnest supporter for seventeen years.

During his long career he contributed over fifty Papers to learned Societies

and Scientific Journals on "Spectrum Analysis," "The Periodic Law," "Electro-Chemistry," and other subjects.

His published Works include (1) "Lectures on Experimental Chemistry" (1874); (2) "General Experimental Chemistry" (1886).

In 1904 he became a Worker in the Davy Faraday Research Laboratory of the Royal Institution, conducting a masterly series of researches on "Silicon and its Compounds: Cooling certain Hydrated Platin-Cyanides in Liquid Air."

He delivered two Friday Evening Discourses at the Royal Institution on (1) "New Alcohols from Flint and Quartz" (1873); (2) "Advances in our Knowledge of Silicon as an Organic Element" (1901).

The Managers desire to express, on behalf of the Members, their sincere sympathy with Lady Reynolds and the family in their bereavement.

The following Lecture Arrangements After Easter 1920 were announced:—

MAJOR G. W. C. KAYE, O.B.E. M.A. D.Sc. Two Lectures on RECENT ADVANCES IN X-RAY WORK. On *Tuesdays*, April 13, 20.

ARTHUR KEITH, M.D. LL.D. F.R.S. F.R.C.S. M.R.I., Fullerian Professor of Physiology, Royal Institution. Four Lectures on BRITISH ETHNOLOGY: THE INVADERS OF ENGLAND. On *Tuesdays*, April 27, May 4, 11, 18.

MAJOR C. E. INGLIS, O.B.E. M.A. A.M.Inst.C.E., Professor of Engineering, University, Cambridge. Two Lectures on THE EVOLUTION OF LARGE BRIDGE CONSTRUCTION. On *Tuesdays*, May 25, June 1.

SIDNEY SKINNER, M.A. M.R.I., Principal, South-Western Polytechnic Institute, Chelsea. Two Lectures on NEW EXPERIMENTAL STUDIES IN THE LIQUID STATE: 1. EBULLITION AND EVAPORATION; 2. THE TENSILE STRENGTH OF LIQUIDS. On *Thursdays*, April 15, 22.

R. CAMPBELL THOMPSON, M.A. F.S.A. Two Lectures on 1. THE ORIGINS OF THE DWELLERS IN MESOPOTAMIA; 2. THE LEGENDS OF THE BABYLONIANS. On *Thursdays*, April 29, May 6.

ALFRED PERCEVAL GRAVES, M.A. F.R.S.L., one of the Founders of the Irish and Welsh Folk Song Societies. Two Lectures (with Musical Illustrations by leading Welsh and Irish Folk Singers) on WELSH AND IRISH FOLK SONG. On *Thursdays*, May 13, 20.

WILLIAM ARCHER, M.A., Dramatic Critic; Author of "India and the Future"; "Through Afro-America"; "Play-Making," etc. Two Lectures on DREAMS WITH SPECIAL REFERENCE TO PSYCHO-ANALYSIS. On *Thursdays*, May 27, June 3.

W. H. ECCLES, D.Sc. M.R.I., Professor of Applied Physics and Electrical Engineering, City and Guilds of London Technical College. Two Lectures on THE THERMIONIC VACUUM TUBE AS DETECTOR, AMPLIFIER AND GENERATOR OF ELECTRICAL OSCILLATIONS. On *Saturdays*, April 17, 24.

FREDERICK CHAMBERLIN, LL.D. F.S.A. M.R.I. Two Lectures on THE PRIVATE CHARACTER OF QUEEN ELIZABETH. On *Saturdays*, May 1, 8.

FREDERIC HARRISON, D.C.L. D.Litt. LL.D., Hon. Fellow and sometime Tutor of Wadham College, Oxford, and late Professor of Jurisprudence, Inns of Court. Two Lectures on 1. A PHILOSOPHICAL SYNTHESIS AS PROPOSED BY AUGUSTE COMTE; 2. THE REACTION AND THE CRITICS OF THE POSITIVIST SCHOOL OF THOUGHT. On *Saturdays*, May 15, 22.

J. H. JEANS, LL.D., Secretary, Royal Society; Author of "The Dynamical Theory of Gases." Two Lectures on RECENT REVOLUTIONS IN PHYSICAL SCIENCE: 1. THE THEORY OF RELATIVITY; 2. THE THEORY OF QUANTA. (The Tyndall Lectures.) On *Saturdays*, May 29, June 5.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Bulletin of Agricultural Research Institute, Pusa, Nos. 87, 89. 1919. 8vo.

Inscriptions of the Madras Presidency. By V. Rangacharya. 3 vols. 8vo. 1919.

Lords Commissioners of the Admiralty—Nautical Almanac, 1922. 8vo. 1919.

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Aeronautical Society, Royal—Journal, Feb. 1920. 8vo.

American Geographical Society—Geographical Review, Oct.-Nov. 1919. 8vo.

Asiatic Society of Bengal—Journal and Proceedings, Vol. XII. Nos. 4–6; Vol. XIII. Nos. 1–5; Vol. XIV. Nos. 1–9; Vol. XV. Nos. 1–3. 8vo. 1916–19.

Australia, Commonwealth of—Science and Industry, Dec. 1919. 8vo.

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Basel, Naturforschenden Gesellschaft—Verhandlungen, Band XXX. 8vo. 1920.

Botanic Society, Royal—Quarterly Summary, No. 3, Jan. 1920. 8vo.

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The Kalendar, 1920. 8vo.

British Astronomical Association—Journal, Vol. XXX. No. 4. 8vo. 1920.

British Dental Association—Journal, Vol. XLI. Nos. 3–4. 8vo. 1920.

Bucarest, Academia Romana—Bulletin, Tome V. Nos. 2-6. 8vo. 1916–18.

Cambridge Philosophical Society—Transactions, Vol. XXII. Nos. 15–18. 4to. 1919–20.

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Carnegie Endowment for International Peace—Year Book, 1919. 8vo.

Signatures, etc., to First and Second Hague Peace Conferences. 8vo. 1919.

Chemical Industry, Society of—Journal, Feb. 1920. 8vo.

Chemistry, Institute of—Journal and Proceedings, 1920, Part 1. 8vo.

Chili, Instituto Meteorologico—Observaciones, 1911–15. 8vo. 1919.

Civil Engineers, Institution of—List of Members, 1920. 8vo.

Metropolitan Road and Rail Transit. By H. H. Gordon. 8vo. 1919.

Colonial Institute, Royal—United Empire, Vol. XI. No. 2. 8vo. 1920.

Devonshire Association—Transactions, Vol. LI. 8vo. 1919.

Editors—Animals' Defender, Feb. 1920. 4to.

Athenæum, Feb. 1920. 4to.

Chemist and Druggist, Feb. 1920. 8vo.

Church Gazette, Feb. 1920. 8vo.

Dyer and Calico Printer, Feb. 1919. 4to.

Engineer, Feb. 1920. fol.

Engineering, Feb. 1920. fol.

Junior Mechanics, Feb. 1920. 8vo.

Law Journal, Feb. 1920. 8vo.

London University Gazette, Feb. 1920. 4to.

Model Engineer, Feb. 1920. 8vo.

Musical Times, Feb. 1920. 8vo.

Nature, Feb. 1920. 4to.

Nuovo Cimento, Jan. 1920. 8vo.

Physical Review, Dec. 1919. 8vo.

Science Abstracts, Jan. 1920. 8vo.

Terrestrial Magnetism, Vol. XXIV. No. 4. 8vo. 1919.

Wireless World, Feb. 1920. 8vo.

Electrical Engineers, Institution of—Journal, Vol. LVIII. No. 287, Jan. 1920. 4to.

Florence, Biblioteca Nazionale Centrale di Firenze—Bollettina, Feb. 1920. 8vo.
Garrett, A. E., Esq., B.Sc. M.R.I. (the Author)—The History and Hygiene of Clothing. 8vo. 1919.
Geographical Society, Royal—Journal, Vol. LV. No. 2. 8vo. 1920.
Geological Society of London—Abstracts of Proceedings, No. 1049. 8vo. 1920.
Greek Bureau of Foreign Information—The Question of Thrace. By J. S. Mills and M. G. Chrussachi. 4to. 1919.
Horological Institute—Horological Journal, Feb. 1920. 8vo.
Iron and Steel Institute—Journal, Vol. C. 8vo. 1919.
Meteorological Office—Professional Notes, No. 9. 8vo. 1920.
Meteorological Society, Royal—Quarterly Journal, Vol. XLVI. No. 193, Jan. 1920. 8vo.
Microscopical Society, Royal—Journal, 1919, Part 4. 8vo.
London County Council—Gazette, Feb. 1920. 4to.
London Society—Journal, Feb. 1920. 8vo.
Pharmaceutical Society of Great Britain—Journal, Feb. 1920. 8vo.
Photographic Society, Royal—Journal, Vol. LX. No. 2. 8vo. 1920.
Rome, Ministry of Public Works—Giornale del Genio Civile, Oct.-Nov. 1919. 8vo.
Röntgen Society—Journal, Vol. XVI. No. 62, Jan. 1920. 8vo.
Royal College of Physicians—List of Fellows, etc., 1920. 8vo.
Royal Cornwall Polytechnic Society—Eighty-Sixth Annual Report. 8vo. 1919.
Royal Dublin Society—Proceedings, Vol. XV. Nos. 35–48. 8vo. 1919–20.
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Royal Society of Arts—Journal, Feb. 1920. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXXIX. Part 3. 8vo. 1919.
Royal Society of London—Philosophical Transactions: A, Vol. CCXX. No. 576; B, Vol. CCIX. No. 368. 4to. 1920.
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Royal Society of New South Wales—Journal and Proceedings, Vol. LII. 8vo. 1919.
Sanderson, The Right Hon. Lord, G.C.B. K.C.M.G. I.S.O. D.C.L. M.R.I.—The Atlantic Telegraph. By W. H. Russell. 4to. 1865.
Sanitary Institute, Royal—Journal, Vol. XL. No. 3. 8vo. 1920.
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Swiss Chemical Society—Helvetica Chimica Acta, Vol. III. Fasc. 1. 8vo. 1920.
United Service Institution, Royal—Journal, Vol. LXV. No. 457. 8vo. 1920.
United States, Bureau of Standards—Bulletin, Vol. XIV. No. 4. 8vo. 1919.
United States Department of Agriculture—Experiment Station Record, Vol. XLI. No. 7. 8vo. 1919.
Journal of Agricultural Research, Vol. XVIII. Nos. 7–8. 8vo. 1919.
United States, Naval Observatory—Annual Report, 1919. 8vo.
United States Patent Office—Official Gazette, Vol. CCLXX.–CCLXXI. No. 2. 8vo. 1920.
Western Australia—Quarterly Statistical Abstract, Nos. 214–215. 8vo. 1919.

WEEKLY EVENING MEETING,

Friday, March 5, 1920.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

The Hon. John N. Fortescue, C.V.O. LL.D.

Military History.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, March 12, 1920.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

W. W. ROUSE BALL, Fellow of Trinity College, Cambridge.

String Figures.

I HAVE chosen as the subject for this lecture *String Figures*, which I present to you as a world-wide amusement of primitive man, and as being in themselves interesting to most people. In the course of the evening you will see how such figures are actually made, but before coming to that I must tell you something of their nature and history. I hope you will bear with me if I introduce them to you in my own way.

A string figure is usually made by weaving on the fingers a loop of string, about six-and-a-half or seven feet long, so as to produce a pleasing design, often supposed to suggest a familiar object, either at rest or in motion.

Having taken up the string in some defined way, the subsequent weaving is usually effected either with the aid of another operator, each player in turn taking the string from the other, or by the single player making a series of movements, such as dropping a loop from one finger, transferring a loop from one finger to another, picking up a string with one finger and then returning the finger to its original position carrying the string with it, and so on; unless I state the contrary it is to be assumed that it is with figures made in the second way that I am concerned to-night. In general, after each step, the hands are separated so as to make the string tight: and normally the hands are held upright with the fingers pointing upwards and the palms approximately facing one another. [These movements were illustrated by the formation of a string figure.] This is all that is required in most constructions, though many other small movements, notably slight rotations of the wrists, while not necessary, give neatness of manipulation and add to the effectiveness of the display.

These figures, when shown to a few spectators in a room, always prove, as far as my experience goes, interesting alike to young and old; but their attractiveness, their fascination I might almost say, is not permanent unless people can be induced to construct them for themselves. I can hardly propose—and that is a difficulty inherent

in lecturing on the subject—I can hardly propose that for the first time, now and here, without individual help you should make the designs you will see later. To enjoy the occupation, however, you must be able to make them, and, bold though I may seem, I venture to assert that if once you acquire this knowledge you will find pleasure in applying it.

It is a truism, and in fact a truth as well, that all sensible people have hobbies. I am not alone in finding that the collection of string figures is an agreeable hobby, and it may be added a very cheap one, while friends who have learnt the rules tell me that in convalescence and during tedious journeys the amusement has helped to while away many a long hour; moreover the figures are easy to weave, they have a history, and they are capable of many varieties. Thus even in England the game may prove well worth the time spent in learning to play it: and admittedly to the very few who travel among aborigines it may sometimes be of real service.

It would be absurd to talk about string figures if you do not know what they are; so before I go any further let me show you what is meant by the term. These figures may be divided into three classes, α, β, γ, according as (α) the production of a design, of (β) the illustration of some action or story, or (γ) the creation of a surprise effect is the object desired; it will be desirable to begin by giving one or two examples of each class.

The designs reproduced in Figs. 1 and 2 are well-known forms which will serve as illustrations of figures in Class α. The first of them, a zig-zag design, termed *Lightning*, is due to the Navaho Red Indians who live on the Mexican border of Arizona, where the customs of the Red Man have not yet been wholly destroyed by

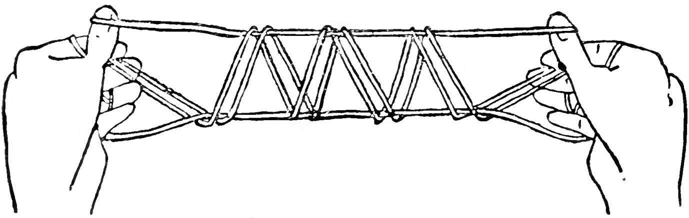


Fig. 1.—Lightning.

civilization and law. [The figure as shown by the Lecturer was made by successive movements, as set out in the next paragraph.] The construction is simple and no digital skill is involved. You see the final result appears suddenly, almost dramatically, and I regard this as an excellent feature of it. Observe also that the production of the figure is rapid. Timing myself, I find I take well under ten seconds to make it. I think quickness, which comes easily as soon

as one knows the moves, adds finish to the working and is worth cultivating.

The movements by which Lightning is produced are easy—a boy of eight or nine will learn them in three or four minutes—but as is the case with all these figures it is difficult to describe them concisely. To illustrate these statements let me express as shortly as I can exactly what I did. *First:* I put the string in the form of a figure of eight, one oval (preferably small) lying away from me, and the other towards me, and the strings crossing in the middle of the figure ; I then put my index-fingers down into the far oval, and my thumbs down into the near oval ; next I separated the hands and then turned them up into their normal position with the thumbs and fingers well spread out, thus causing the strings of the loops on the thumbs and index-fingers to cross one another. *Second:* I bent each thumb away from me over two strings, and with its back picked up from below the next string (i.e. in the language expounded later the ulnar index string), and, as usually follows and is assumed to be the case unless the contrary is stated, returned the thumbs to their former positions. *Third:* I bent each mid-finger towards me over one string, and with its back picked up from below the next string. *Fourth:* I bent each ring-finger towards me over one string, and with its back picked up from below the next string. *Fifth:* I bent each little-finger towards me over one string, and with its back picked up from below the next string. *Sixth:* I moved my thumbs away from me, and placed their tips in the spaces by the little-fingers, their fronts resting on the near little-finger string : this released the thumb loops. *Finally:* I threw the loops thus released over the other strings, and at the same time with the thumbs either pressed up the far little-finger string or pressed down the near little-finger string, and the figure flashed out. The movements are really simple, though the description is lengthy, but in my opinion it is not desirable to labour at making this extremely concise.

The next diagram is of a design, known as a *Tent-Flap* or *Door*, due to the Apache Red Indians. [The figure as shown by the Lecturer was made by successive movements, as set out in the next paragraph.] The Apaches are now almost extinct, but the figure is familiar to the Mexican Indians, who are said to have learnt it from Apaches living on the Reservation Lands maintained by the United States Government. This also is a figure in Class *a*.

[*The Apache Tent-Flap.*—The successive movements which produced the result shown in the Lecture may be put in the form of the following rules :—*First:* Take up the string in the form of Opening A.* *Second:* Lift the loops off the index-fingers, pass them over their corresponding hands on to the wrists, thus making them dorsal strings. *Third:* Bend each thumb away from you over one

* This term is explained below, on page 93.

string, and with its back pick up from below the next string, and return. *Fourth:* Bend each little-finger towards you, and with its back pick up the next string. *Fifth:* Grasp with the left hand all the strings in the centre of the figure where they cross, pass this bunch of strings from the palmar side between the right thumb and index-finger so that the bunch lies along the arm, with the left thumb and index-finger take hold of the two loops on the right

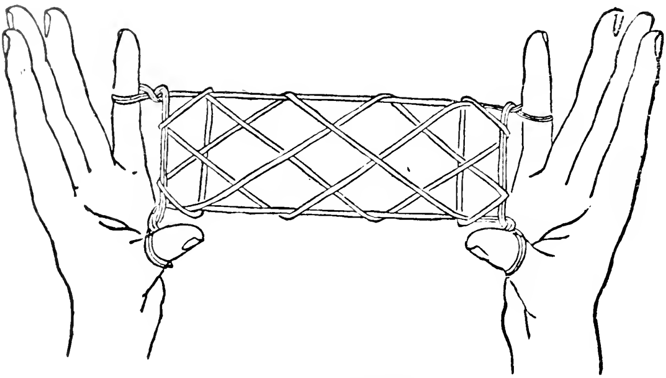


FIG. 2.—A TENT-FLAP.

thumb, draw them over the tip of the right thumb, let the bunch of strings also slip over to the right thumb to the palmar side, and then replace the two loops on the right thumb; make a similar movement with the other hand. *Lastly:* Lift the wrist loops over the hands, letting them fall on the palmar sides, rub the hands together, separate the hands, and the figure will appear.]

The two designs, represented in Figs. 3 and 4, will serve as examples of figures in Class β. The first of them is supposed to

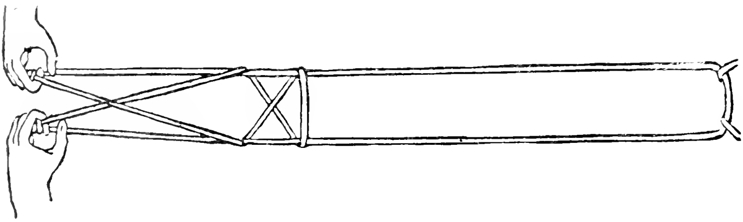


FIG. 3.—A MAN CLIMBING A TREE.

represent a *Man Climbing a Tree*, his arms and feet (or perhaps his feet and tree-band) clasping the tree trunk. It is derived from the Blacks in Queensland; since only a drawing of the design was

brought away, it is impossible to be certain how it was made by the aborigines, but the construction I am about to employ has been suggested, and is probably correct, since it is simple and involves no unusual actions. [The figure as shown by the Lecturer was made by successive movements, as set out in the next paragraph.] In the figure thus obtained I pull with my index fingers, and then the part which represents the man moves up the part which represents the tree trunk. Such motion is characteristic of figures of this kind: hence such results are often used as a framework for stories—two warriors fighting, a hammock breaking and its occupant falling out, and so on.

[*A Man Climbing a Tree.*—The successive movements which produced the result shown in the Lecture may be put in the form of the following rules:—*First:* Take up the string in the form of Opening A.* *Second:* Bend each little-finger towards you over four strings, with its back pick up the next string, and return. *Third:* Navaho* the little-finger loops. *Fourth:* Bend each index-finger over the palmar string between the two strings of the loop on the corresponding index-fingers, and press the tips of the fingers on the palms. *Fifth:* Holding the strings loosely, slip the loops off the thumbs and index-fingers. *Lastly:* Put the far little-finger string under one foot, or under a heavy book, release the little-fingers, and pull steadily with the index-fingers, after hooking their tips into the string they hold.

Closely allied to the production of moving figures, and almost indistinguishable from them, are String Illustrations of Stories. The well-known representation of the *Yam Theft* will serve as an example of this type of figure. [The construction as shown by the Lecturer was made by successive movements, as set out in the next paragraph, the final form being shown in the diagram on next page.] You can tell the story much as you like. In one version of it the thumb loop represents the owner of a yam patch. He is supposed to be asleep. The loops successively taken up from the dorsal string and put on the fingers represent the yams dug up by a thief, and tied up in bundles ready for carrying off. The loop coming off the thumb represents the owner waking and going to see what is the matter. He looks down the dorsal side, sees the yams collected for removal, notices that the dorsal strings hold them tight, and looks about for the thief. The thief, who may be represented by a loop on the pendant palmar string, coming back for his booty, sees the owner, whereupon (pulling the pendant palmar string) he disappears with all the yams. There is at least one British specimen of such a string story which deals with the misadventures of a thief who stole some tallow candles. I include these string illustrations of stories among the figures in Class β.

* This term is explained below, on page 93.

[*The Yam Theft.*—The successive movements which produced the figure shown in the Lecture may be put in the form of the following rules:—*First:* Hold the left hand open with the palm facing you, the thumb upright and the fingers pointing to the right and slightly upwards. With the right hand, loop the string over the left thumb, crossing the strings if you like, and let one string hang down over the palm and the other over the back of the hand—we may call these the palmar and the dorsal strings. *Second:* Pass the right index-finger from below under the palmar string, and then between the left thumb and index-finger, and with its front tip hook up a loop of the dorsal string. Pull this loop between the left thumb and index-finger back on to the left palm. Then with the right index-finger give the loop one twist clockwise, and put it over the

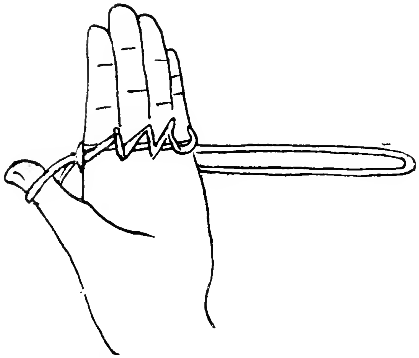


Fig. 4.—The Yam Thief.

palmar string on to the left index-finger. Pull the two pendant strings so as to tighten the loops on the thumb and index-finger. *Third:* In the same way pass the right index-finger from below under the pendant palmar string, and then between the left index and middle fingers, and with its front tip hook up another piece of the pendant dorsal string: pull this loop back on to the left palm, and with the right index-finger give the loop one twist clockwise, and put it over the palmar string on the left mid-finger. *Fourth:* In the same way, working between the middle and ring fingers, hook up another loop of the pendant dorsal string, and put it on the left ring-finger. *Fifth:* In the same way, working between the ring and little fingers, pick up another loop of the pendant dorsal string, and put it on left little-finger. *Sixth:* Take off the left thumb loop, and hold it between the left thumb and index-finger; and, for the sake of effect, to show that the loops are still on the fingers, pull the pendant dorsal string. *Finally:* Pull the pendant palmar string, and the figure will come off the hand.]

There is yet a third class, which I call Class γ, of string figures to which primitive man is very partial: these are string paradoxes, where the unexpected happens. Take this as an example. Here is a loop of string, held for convenience by my left hand high up. Obviously if I twist my right hand round one string of the loop and pull with the left hand, the right hand will be caught. If I give the right hand a twist round the other string of the loop, it is generally still more firmly caught. The problem is to give this additional twist so that the string runs free when the left hand is pulled. This can easily be effected by what is known in certain South Pacific Isles as the *Lizard Twist*. [This was shown.] There is no trickery; the movements are simple, yet I predict that few people, even if they have seen the twist, will succeed when they first attempt to make it. String paradoxes or puzzles of this kind are widely known, and are generally amusing. To show them, to be shown them, and above all to show pleasure in them, often lead to friendly intercourse with primitive folk, but they are different in kind from the figures about which I wish to talk. I put them, then, on one side as not relevant to my subject to-night, and come back to the formation as practised to-day of string designs in Classes α and β.

[*The Lizard Twist.*—The successive movements made in the Lizard Twist may be put in the form of the following rules:—*First:* Hold the string rather high in the left hand, the string hanging down on the right and left sides in a loop. *Second:* Put the right hand with its fingers pointing down and away from you through the loop: turn it round the right pendant string clockwise. *Third:* Point the fingers upwards, move the right hand to the left between your body and the pendant strings, then clockwise beyond the left pendant string, then away from you, then to the right, and finally towards you upwards through the loop. *Lastly:* Draw the right hand down and to the right, and it will come free.]

The study of string figures is new, and its history a short one. I may dispose of the story prior to 1900 very briefly. From about the middle of the nineteenth century onwards we find occasional notices by travellers in wild countries of the fact that the natives made, with a piece of string, forms different from and far more elaborate than the Cat's Cradle of our nurseries, but (with the exception of two examples described in France in 1888 and two in America in 1900) no details were given of how they were constructed, and in only a few cases near the end of the century were drawings kept of the patterns produced. There are more accounts of the Cat's Cradle familiar to children in England; indeed they go back to the eighteenth century, for there is an allusion to it in English literature as long ago as 1768, and Charles Lamb refers to it as played at Christ's Hospital in his school-days. It is, however, a dull amusement, producing, as usually presented, merely four or five designs of little

interest: here, too, before the present century, no description was available which would enable anyone previously ignorant of the Cradle to make it. Outside Britain, in the nineteenth century it was known in Northern Europe, and travellers in Victorian times mention it as practised in Korea, China, and the Asiatic Isles.

We may say that before 1900 the whole matter of string figures was regarded as a pastime of children and savages, hardly worth mention and not worth consideration. To-day, when serious attention is given to folk-lore and the histories of games, such things are looked at from a different stand-point. The study of string figures came about in this way. In 1898 Haddon organised an anthropological expedition to the Torres Straits, and among other things brought back information about string patterns there current, together with some thirty examples. Some of these designs were made to the chanting of sing-songs, some were connected with tribal stories, and some were amusements, but everything suggested that here was a custom worth investigation.

This conclusion showed the need of having an unambiguous nomenclature which would allow anyone acquainted with it to describe a string figure in such a way as to permit of its reproduction by an intelligent reader. The terms introduced are taken from anatomy, and there is nothing recondite about them, but it is necessary to know them if you want to understand recent writers on the subject. Here they are:—

The part of a string which lies across the palm of the hand is described as *palmar*, the part lying across the back of the hand as *dorsal*.

Anything on the thumb side of the hand is called *radial*, anything on the little-finger side is called *ulnar*. Since a string passing round a finger or fingers forms a loop, each such loop is composed of a radial string, and an ulnar string.

Of two strings or loops on the same finger, the one nearer the palm of the hand is called *proximal*, and the one nearer the finger tip is called *distal*.

These six adjectives, palmar and dorsal, radial and ulnar, proximal and distal, together with the names of the parts of the hands, fingers, wrists, etc., enable us to state exactly the relative place of every string in a figure held on the hands.

This nomenclature is framed so as to define the position of strings on a hand by reference to the hand, and not by terms like near and far, lower and upper, which may mean quite different things according as to how it is held. At the same time, if the hands are held upright and with the palms facing each other, which I regard as their normal position, we may conveniently use *near* and *far* instead of radial and ulnar, and *lower* and *upper* instead of proximal and distal. It is well however to make it an absolute rule that these every-day words are used only when the hands are in their normal position.

This precision of language, which was necessary if the subject was to be treated scientifically, was introduced in 1902. Subsequent research has strengthened the interest taken in string figures, and in anthropological expeditions to-day they are among the matters on which information is sought. In particular Haddon has continued to stimulate enquiry, and to him we owe many of the patterns discovered. It is not too much to say that he is the creator of the science, and to his enthusiasm and knowledge many of us owe our introduction to it.

The Americans took up the investigation warmly, and in Philadelphia a valuable collection of drawings of string figures has been formed which will preserve permanently many of the patterns discovered. The results of the earlier work in America are embodied in a handsome volume* published in New York in 1906, containing full descriptions of about a hundred string figures, chiefly collected in North America and New Guinea, though with some examples from Africa, the Philippines, and other scattered localities. In it also are given drawings of more than a hundred finished patterns from Oceania and Queensland. Unfortunately Mrs. Jayne, to whose liberality and initiative the book was due, died shortly after its publication.

Further examples from places where the amusement was already known to exist, and collections from Africa and India, have since been issued, and show that the construction of string figures is widely practised where primitive man is still found. Examples also have been reported from South America, but as yet this immense area is an almost unworked field, the only well-known South American instance being a *Fly*—an example of Class β. [The Fly can be made thus:—(1) Put the thumbs, held upright, into the loop and draw tight. (2) Move the left hand to a horizontal position; then turn it counter-clockwise under the strings and up towards you into its normal position, thus giving two dorsal strings. (3) Pass the right hand between you and the left hand, then put the right little-finger from above under the dorsal strings, pick them up, and return. (4) Pass the left hand between you and the right hand, then put the left little-finger on the palm, and pass it towards you under the two strings on the right thumb, pick them up, and return. (5) Lift the left dorsal string over the digits, and extend. This is the Fly. Next its proboscis (or some part of its anatomy) is shown by releasing the little-fingers. To try to catch the fly, clap your hands together: on drawing them apart quickly and as far as possible, it will always be found that the fly has escaped, in fact the display of the proboscis destroyed the figure. (F. E. Lutz, "String Figures of the Patomana Indians on the Northern Brazilian Frontier," Anthropological Papers, Amer. Mus. of Nat. Hist., vol. xii.,

* "String Figures," by C. F. Jayne. New York, 1906.

New York, 1912.) Ethnologists, more conservative than primitive men, deem it undesirable or worse to vary recorded methods, so with hesitation I add that the Indians might have made the conclusion more effective by not displaying the proboscis and thus not destroying the fly as a definite creation: in this case, as before, on trying to squash it, you clap your hands sharply together, then drawing them apart quickly and at the same time releasing the little-fingers, the fly will have disappeared.]

In 1911 Miss Haddon* published in London an excellent popular account of some of the results available, with typical designs; so the amusement is now open to all willing to learn the moves. Later, in 1914, Dr. Hambruch printed at Hamburg a long memoir on the subject, with special regard to the patterns found at Nauru in Micronesia, the home of some of the most skilful native exponents of the art, and then a German possession. Authorities for all the figures I am making to-night, except the Fly, will be found in Jayne or Haddon. Of course the outbreak of war in 1914 put a stop to researches of this kind, as of so many others. Hence the serious study of the subject covers only twelve years—namely, from 1902 to 1914—and as yet few save specialists know much about it: but materials increase rapidly, and the number of recorded specimens, which in 1902 was less than fifty, already runs to many hundreds.

I may sum up the result of the work of these twelve years by saying that the evidence does not justify us in asserting dogmatically that all primitive people play and always have played at making string figures; but we may say that the game was at one time common among a large number of them. The formation of these designs is natural, for there are not many sedentary occupations open to uncivilized man during his long leisure hours, and to toy with a piece of string is an obvious way of occupying time. What, however, is striking, is the immense variety of well-defined patterns already discovered, and their distribution in different parts of the world.

The search for and collection of designs was begun only just in time. With the advance of civilization, games such as these are apt to be discarded by adults, and survive only among the children. I suspect that this is why, until recent times when Cat's Cradle was imported from Asia, there were in European literature, covering many centuries of cultured life, no allusions to string figures.

Among existing aborigines, it is usually the women who teach the pastime to the children, and in most cases nowadays the lads and men, though familiar with the methods used, do not of their own accord make designs in the presence of strangers. Hence the amusement may easily escape the attention of travellers; no doubt, also, many of these would take no interest in such figures even if they saw them. Moreover, in wild countries the natives are shy, and

* "Cat's Cradles from Many Lands," by K. Haddon, London, 1911.

think that the white man will laugh at these simple games; thus an exhibition is not made unless encouraged by sympathetic advances, but if patterns are shown no secret is made about the method of construction, which is not treated as a tribal secret. To this open revelation of methods of weaving there is one reported exception mentioned by Boas, and referred to later. Even, however, when figures are displayed, it does not follow that it is easy to take down or follow the rapid sequence of moves made by the operator, so the collection of records may involve a good deal of gentle diplomacy.

I can give you an illustration of this reluctance to show figures unless they are asked for. Haddon, when a few years ago near the Victoria Falls in Africa, met a high official of the Government, and, enquiring about various customs of the natives, asked if any string games were known in that part of the country. The officer said, "No"; he had never heard of them, he had lived for years among these people, had constantly seen them at work and at play, and was confident that nothing of the kind could exist without his knowledge. After their talk Haddon strolled over to where the police escort waited, and taking out of his pocket a piece of string (without which to-day no self-respecting anthropologist ought to travel), made to their obvious pleasure a couple of string figures. He then tossed the string to a black orderly, who made other patterns. In fact these natives were acquainted with various forms, and when Haddon, disguising his deeper knowledge, showed interest, they were delighted and readily exhibited to him such designs as they knew. One of these is worth reproducing here, for it represents (what is rare in such designs) a place, namely the Batoka Gorge on the Zambesi River below the Victoria Falls. [The figure as shown by the Lecturer was made by successive movements, as set out in the next paragraph.]

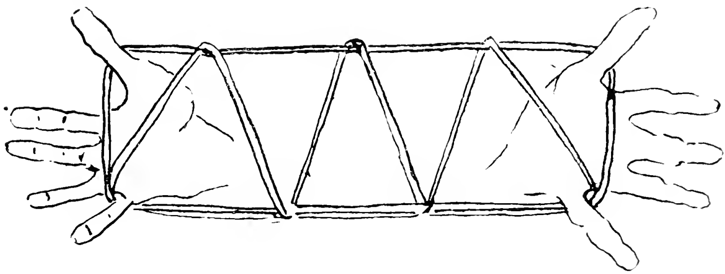


FIG. 5.—THE BATOKA GORGE.

This incident suggests, what I believe is the truth, that the best way of finding out native figures is to make some oneself, and then challenge the natives to do better if they can: for this, no extensive acquaintance with their language is necessary, a very obvious advantage in opening communications on so technical a matter.

[*The Batoka Gorge* as shown by the Lecturer was made by the following successive movements:—*First:* Hold the right hand horizontal, pointing away from you and with its palm facing downwards: rest the string on the right wrist so that two equal loops hang freely down, one on its radial side, the other on its ulnar side. *Second:* Pass the left hand from left to right through both loops, and bring both hands into their normal positions. *Third:* Bend each little-finger towards you, and with its back pick up both the strings which cross each other in the centre of the figure. *Fourth:* Throw the near wrist string away from you over both hands to their far side. *Fifth:* Bend each thumb away from you, and with its back pick up the corresponding oblique near little-finger string. *Lastly:* Take each far wrist string and (keeping the other strings unaltered in position) pass it over the hands to the near side of the wrists. Extend the hands, and the figure, representing a bird's-eye view of the zig-zag course of the river through the gorge, will appear.]

Apart from collectors, who naturally find pleasure in getting specimens of what they collect, travellers in uncivilized countries, even if uninterested in string figures, will find some knowledge of them a useful equipment. A native is apt to distrust a missionary, a prospector, and a trader; but a stranger, who exhibits what may well be taken to be one of the innocent games of his own people, offers credentials to which a friendly response is, as far as experience goes, invariably made. Who, indeed, would attribute evil intentions to one who comes armed only with a piece of string, and seems chiefly interested in amusements similar to those familiar to the onlookers in their childhood? This is not a matter of mere conjecture. I know of at least one definite instance where cordial relations were thus at once established.

Of course from the beginning of the study of these figures the question arose of their possible relation to historical and religious traditions. Up to now, however, with the exception of a few isolated facts, no evidence of such connection has been found. Indeed the only traces of it so far recorded are that in New Zealand the forms are associated with mythical heroes, and the invention of the game attributed to Maui, the first man; that various designs common to many of the Polynesians are often made to the accompaniment of ancient chants; that the Eskimo, too, have songs connected with particular patterns, have a prejudice against boys playing the game for fear it should lead to their getting entangled with harpoon lines, and hold that such figures, if made at all, should be constructed in the autumn so as to entangle the sun in the string and delay the advent of the long winter night. Further, Boas asserts that among the Kwakiletl of Vancouver Island the form known as "Threading a Closed Loop" is used instead of a password by members of a certain secret society to recognise fellow-members. These facts, interesting though they be, do not come to much, and it would seem

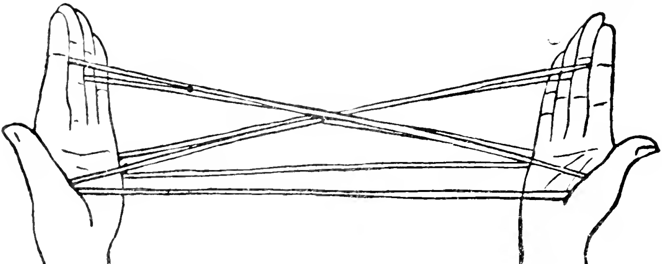
that as yet there is no substantial evidence that the construction of string figures is other than a recreation. I say "as yet," for new discoveries may at any time alter our views on this question.

Now let me put aside these historical questions, and consider the patterns actually made and their making. In opening the subject I remarked that for constructing string figures two methods are commonly applied ; these are known respectively as the *Asiatic* and the *Oceanic*. In the former, two players are required, of whom one at each move takes the string from the other ; in the latter, normally, only one player is required, who weaves the pattern with his fingers, using, if need be, his feet and teeth to assist him.

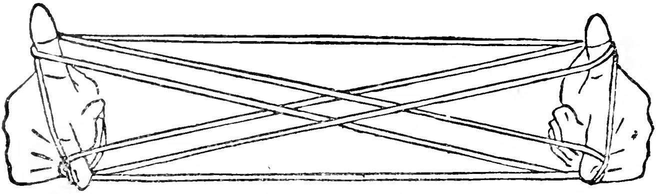
The Asiatic method lends itself to many varieties, but as far as I am aware these have not been developed, and broadly speaking this method is known to us almost only in the classical form, common in the English nursery, of Cat's Cradle. This form occurs in Korea, Japan, the Asiatic Islands, China, and Northern Europe, and the result is a figure of Class α. The weaving begins by the first player twisting the string round the four fingers of each hand, so as to make two dorsal strings and one palmar string : next picking up the string on the palm of each hand with the back of the mid-finger of the other hand, and then drawing the hands apart. The four fundamental figures, which can be made in succession, are known in England as the Cradle, Snuffer-Trays, Cat's-eye, and Fish-in-a-Dish. These are shown in the diagram on page 90 ; the method of construction is widely known, and I need not display it here. Another figure, called a Pound of Candles, is usually (though unnecessarily) interpolated : a few other designs and an arrangement for a See-Sawing movement can also be introduced. That is all. In Korea the four fundamental figures are known as a hearse-cover, a chess-board, a cow's-eye, a rice-pestle, and the interpolated figure as chop-sticks. In other places other names are given.

I do not propose to describe Cat's Cradle further. As usually played, it leads only to a fixed sequence of four or five forms ; no skill is required, and there is little opportunity for variety. Probably to-day ethnologists are the only people of mature age who concern themselves with it. It is believed to have had its origin in Eastern Asia, and to have been thence conveyed to Northern Europe, perhaps by tea traders. A map of the localities in which it is practised shows a band of marks along the east and north of Asia and the north of Europe. From England, with its unceasing output of emigrants, missionaries, and venturers, it has probably been carried to other localities, but I do not think it is common outside the countries I have named.

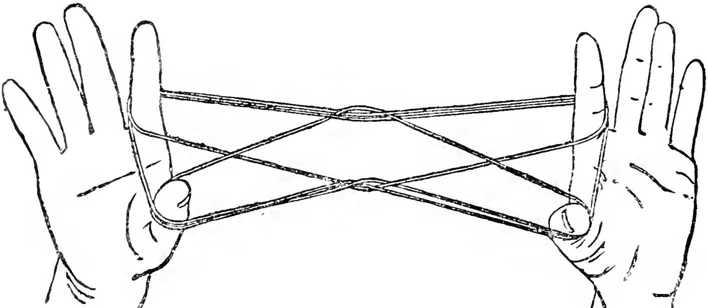
Oceanic examples of Classes α and β are more interesting, and far more widely spread. They occur among the Eskimo, and the natives in America (North and South), Oceania, Australasia, Africa, and India, though the last-named country, as we might expect from its



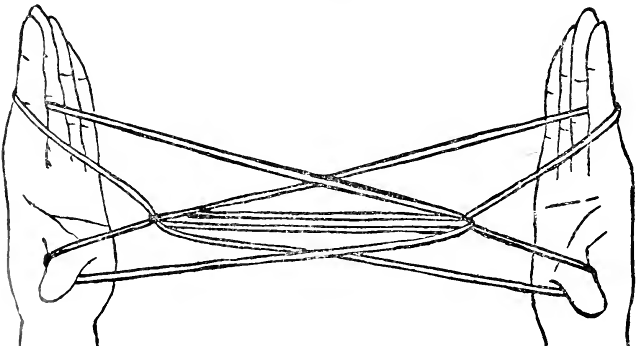
The Cradle.



Snuffer-Trays.



Cat's-Eye.



Fish-in-a-Dish.

FIG. 6.—CAT'S CRADLE: THE FOUR FIGURES.

ancient civilization, has not given us many designs. In this form there is almost invariably only one player. The figures produced are numerous, and many of them can be made, and are made, in more than one way. In this country only one Oceanic construction, known as the *Leashing of Lochiel's Dogs*, has been discovered. [The figure as shown by the Lecturer was made by successive movements, as set out in the next paragraph.] This, in some places termed *Crow's*

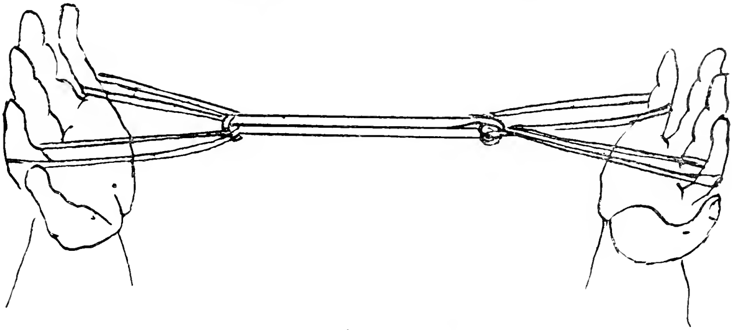


Fig. 7.—Crow's Feet.

Feet, is the most widely distributed of string designs as yet catalogued, occurring in Africa, Australasia, the Pacific Isles, and America. It may be indigenous in Great Britain, but in a sea-surrounded land like this, having ship communication with all parts of the world, it seems more likely that it is an importation.

[*Crow's Feet.*—The successive movements which produced the result shown in the Lecture may be put in the form of the following rules:—*First:* Take up the string in the form of Opening A.* *Second:* Insert the four fingers of each hand from above into the corresponding thumb loops, and throw the near thumb string over the backs of the hands. *Third:* Transfer each index-finger loop to the corresponding thumb. *Fourth:* Transfer each dorsal loop to one of the free digits of that hand, for choice I prefer the index-finger. *Fifth:* Pass each near little-finger string from below through the corresponding index-finger loop, place it on the far side of the little-finger, and Navaho* the far little-finger strings. *Lastly:* Release the thumbs and extend. In the working of this figure in different places there are many small variations.]

Recently I came across an instance of how such figures may be introduced here. A friend of mine, then living at an inland town, showed me a well-known figure, sometimes called a *Fishing Net*, sometimes *Quadruple Diamonds*. [The figure as shown by the Lecturer was made by successive movements, as set out in the next

* This term is explained below, on page 93.

paragraph.] Examples of this have been found in Africa, Oceania and America, but it was said to be unknown in Europe. This he had learnt here in boyhood, and therefore supposed it to be an English production. On enquiry we found that his nurse had taught it to him, and as a result of further talk it seemed that she had got it from a sailor to whom she had been engaged to be married; the conclusion that the latter had learnt it in the course of his voyages

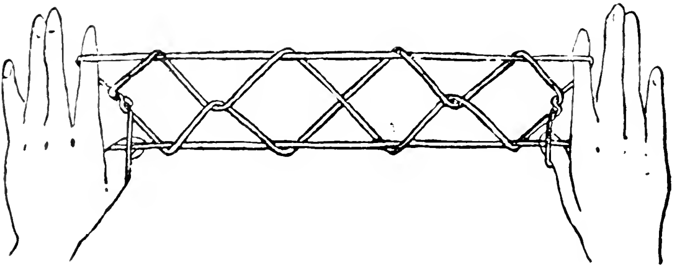


Fig. 8.—A Fishing Net, or Quadruple Diamonds.

seemed a safe one. The figure in question is typical of the numerous patterns made of diamond-shaped lozenges, strung between two parallel strings, arranged either in single rows (of one or two or more, as the case may be), or in the form of rows side by side as in Fig. 2.

[*A Fishing Net.*—The successive movements which produced the result shown in the Lecture may be put in the form of the following rules:—*First:* Take up the string in the form of Opening A.* *Second:* Release the thumbs, then bend them away from you under four strings, and with their backs pick up the far little-finger string, and return. *Third:* Bend each thumb away from you over one string and with its back pick up the next string. *Fourth:* Release the little-fingers, then bend each of them towards you over one string, and with its back pick up the next string. *Fifth:* Release the thumbs, then bend each of them away from you over two strings, and with its back pick up the next string. *Sixth:* Pick up from the base of each index-finger the near index string, and put it over the corresponding thumb, and Navaho* the thumb loops. *Seventh:* Put each index-finger from above into the adjacent palmar triangle, whose sides are formed by the radial little-finger (palmar) string twisting round the thumb loops. *Lastly:* Rotate the right hand counter-clockwise and the left hand clockwise (in doing which the little-finger loops and the proximal index loops will fall off, the palms of the hands will face outwards and away from you, the thumbs will be below the palms and point away from you, and the index-fingers will point upwards), and separate the hands.]

* This term is explained below, on page 93.

A remarkable feature in the Oceanic examples is that a majority of the figures begin in one way. In this the tips of the thumbs and little-fingers of each hand are put together, and then from below into the loop of string; next the digits are separated, and the hands drawn apart (this is called the First Position); and, lastly, the palmar loop on each hand is picked up by the back of the index-finger of the other hand: this is known as *Opening A* or *B*. The fact that such a normal (and not very obvious) opening exists all over the world suggests either that the game was played by the ancestors of the existing races before they were widely dispersed, or that in the long series of past generations there has been more occasional intercourse between natives of distant localities than was formerly suspected, and of course a single stray voyager, whether travelling on his own initiative or driven from home by some unhappy chance, might serve to carry with him the methods of making such figures traditional among his own folk. Either view implies a long history, perhaps extending over thousands of years.

In *Opening A* the left palmar string is taken up before the palmar string. If the right palmar string is taken up by the left-index finger before the left palmar string is taken up by the right-index finger it leads to *Opening B*. In most Oceanic figures it is immaterial whether we begin with Opening A or Opening B.

There is also another movement which is made in the construction of many figures. This is when we have on a finger two loops, one proximal and the other distal, and the proximal loop is pulled over the distal loop, then over the tip of the finger, and then dropped on the palmar side. This movement is not uncommon; it was first discovered among the Navaho Indians: hence it is called *Navahoing the Loop*. I describe the process as *Movement T*.

And now having talked at large about the subject, I want to spend the remaining ten minutes in showing you a few of the more interesting and less common of these Oceanic figures. I had originally intended to make some myself, and use lantern slides of natives displaying others; but I can do better, for Mrs. Rishbeth, who has accompanied her father, Dr. Haddon, in one of his adventurous expeditions, and herself is among the most skilful exponents of the art of making these figures, has most kindly consented to come to London to show us various examples, most of which have never before been exhibited in public. At the beginning of the Lecture I called your attention to the fact that quickness is a desirable feature in the game, and it is not difficult when once you know the moves. The designs I have made in the course of the Lecture—all of them well-known—are easily made, and none took fifteen seconds to complete, though of course were any used as a basis of the story the construction would probably occupy more time. Most of those which Mrs. Rishbeth will make are more involved, but you will notice that in the majority, notwithstanding the many moves

required. Mrs. Rishbeth's working of a design will be completed well within twenty seconds, and even the three or four elaborate constructions, such as the Crab, the Duck, the Alaskan River, and the Porker will take less than one minute.

[Mrs. Rishbeth then showed fourteen examples of string figures, six being in Class α and eight in Class β. The following are examples in Class α: the *Fish Spear*, the *Crayfish*, the *Rabbit*, the *Crab*, a *Tree Burial*, and the *Duck*. The following are examples in Class β: a *Man paddling his Kayak*, the *Eel*, the *Tide*, an *Alaskan River*, the *Looper Caterpillar*, the *Scrub Hen*, the *Frog*, and the *Porker*. Her drawings and the descriptions, in her own words, of her workings are given in an Appendix.]

In selecting these constructions as the subject of this Lecture I have been venturesome, but I plead guilty to liking to wander in the outlying fields of science, and, as I have found pleasure in String Figures and their history, I hoped that others might do the same.

[W. W. R. B.]

APPENDIX.

1. The Fish-Spear = *Baur*. (Murray Island, Torres Straits.)

Position I.

Take up with the right index proximally the left palmar string, twist it once and return. Pass the left index through the right index loop from the distal side, pick up the right palmar string proximally and return.

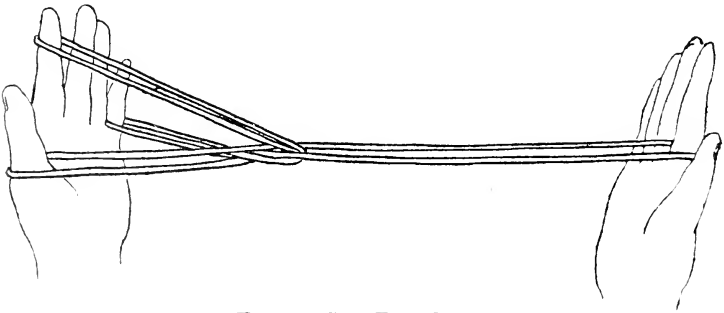


Fig. 9.—The Fish Spear.

Release right thumb and little finger and draw the hands apart.

This is one of the most widely distributed of figures, occurring

over British New Guinea and Torres Straits,* and has also been described from British Columbia and Vancouver Island.

2. THE CRAYFISH.† = *Kaiaru.* (Kiwi, Fly River, Papua.).

Opening A.

Pass thumbs proximal to the index loops and into the little-finger loops from the proximal side, turn them down away from you and up towards you, thus picking up the ulnar little-finger string. Release little finger.

Pass the middle, ring and little fingers proximally into the index-finger loop, and hold down the radial string, pass the indices distally into the thumb loops and turn them down away from you and then up, picking up the two ulnar thumb strings through the original index loop.

Place the radial index string in the centre of the figure, over the toe.

Pass the little fingers proximally into the triangles adjacent to the thumbs, whose bases are formed by the radial thumb string, and hook down this string.

Lift the toe strings off the indices, release thumbs and extend.

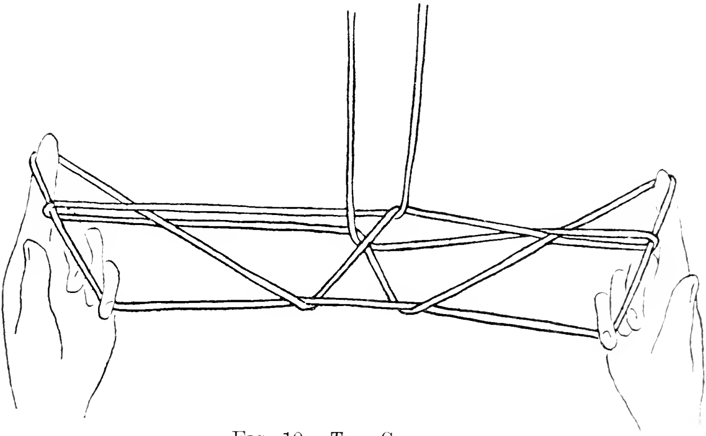


FIG. 10.—THE CRAYFISH.

* Originally described by Rivers and Haddon, "A Method of Recording String Figures and Tricks," Man, October 1902, cix, p. 146. See also "Cat's Cradles from Many Lands," by Kathleen Haddon, Longmans, Green and Co., London, 1911, p. 7.

† Except where otherwise stated the figures were collected by myself.

3. The Rabbit.* (Chukchi, N.E. Asia.)

Hang the string over the left thumb so that there is a loop about two inches long on its radial side.

Put the right thumb and index into the short loop from the radial side, encircle the long loop and insert them into this loop from its ulnar aspect, and return through the short loop.

Grasp the two long strings with the right little finger below the short loop and rotate the left thumb once clockwise.

Pass the left little finger distal to the strings, to the radial side of the right thumb loop and hook it round the radial thumb string from the proximal side, release right thumb.

Place the right index tip to tip with the left thumb, and transfer the index loop to the thumb.

Navaho thumb and extend.

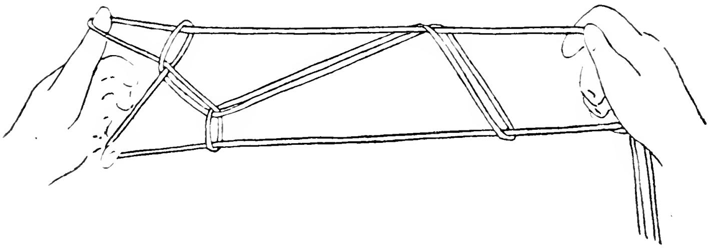


Fig. 11.—The Rabbit.

4. The Crab. = *Ka-ouri*. (Goaribari, Gulf of Papua.)

Opening A.

Insert each index into the little-finger loop from the distal side; bend it towards you and pass it to the proximal side of the radial little-finger string, and bring it back to its original position by passing it between the ulnar thumb string and the radial index string. Release little fingers.

With little fingers hook down the two ulnar index strings.

(Release thumbs gently and insert them again into the same loops in the opposite direction.) Pass thumbs to the palmar side of the strings, passing obliquely from the radial side of the index fingers to the ulnar little-finger strings and pick it up on their backs.

Transfer thumb-loops to little fingers.

Pass thumbs away from you through little-finger loops and to the palmar side of the double strings running from index to little finger. With backs of thumbs take up these strings, returning through little-finger loops.

Release little fingers and transfer thumb-loops to little fingers.

* Collected by my sister, Mrs. A. E. Hodder.

A straight string passes between the indices. With the thumbs take up this string from the proximal side close to the indices. Release indices.

Put indices into thumb-loops towards you and withdraw thumbs.

*

A loop passes from the centre of each palmar string to the outer angle of the central lozenges; take up with the thumbs from the proximal side the string of this loop that lies nearest to you.

Bring the thumbs together, tip to tip, and exchange the loops, the left passing under the right.

Pass the middle fingers distal to the index loops and take up the ulnar thumb-strings from the proximal side. Release thumbs, pass them into the middle-finger loops from the distal side and pick up the ulnar middle-finger string from the proximal side. Release middle fingers (i.e. change direction of thumb loops).

With the thumbs take up proximally the radial index strings and return through the thumb-loops, allowing the original thumb-loops to slip off.

Release indices and transfer thumb-loops to indices.

One of the two radial little-finger strings of each hand goes across the figure and crosses the corresponding string from the other little finger in the middle within the central triangle. Take up these strings from the proximal side at the point where they cross the triangle with both thumbs, so that there is a double string running from thumb to thumb.

With the thumbs take up the radial index strings proximally and return through the thumb-loops, allowing the original thumb-loops to slip off. Release indices, transfer thumb-loop to indices.

Repeat from *

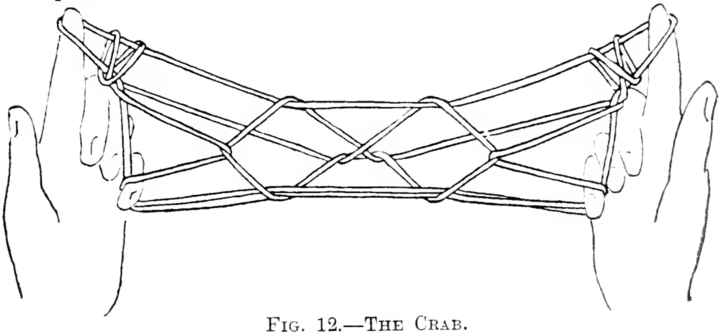


FIG. 12.—THE CRAB.

The Crab is then shown with its open nippers held up, and thus is a slight improvement on the one originally described.*

* Rivers and Haddon, *loc. cit.*

5. Tree Burial. (Buneki, Mouth of Bamu River, W. Papua.)

Opening A.

Do the seven succeeding paragraphs of "The Crab" (see p. 96), i.e. as far as "Release indices," but omitting the movement in brackets at the beginning of the third.

There is a loop catching down the string running between each thumb and little finger: pass each index towards you through its respective loop so that the longer oblique string is on its radial side; draw this string out away from you and pass the index fingers over (radial to) the radial thumb string; turn the indices down and up away from you, thus picking up the radial thumb string.

Release thumbs, but do not draw tight. With the thumbs lift up each radial little-finger string, thus raising a central string running the length of the figure.

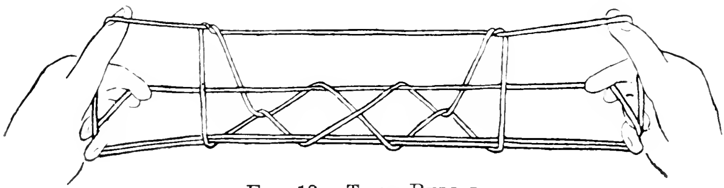


Fig. 13.—Tree Burial.

This represents the platform where the body is placed in a tree, with a rude awning over it. Or as the native who taught it to me described it: "Dead man he sleep."

6. The Duck * = *Welchu.* (Chukchi, N.E. Asia.)

Position I.

With the right thumb pick up the left ulnar little-finger string from the proximal ulnar side; do not draw tight. With the left thumb pick up the right ulnar little-finger string from the proximal radial side. Extend figure and release little fingers.

With the little fingers hook down the ulnar thumb strings.

Pass the indices proximal to the oblique radial thumb strings and distal to the straight radial thumb string, turning the index fingers down and up away from you pick this string up on their tips. Release thumbs.

Pass the thumbs into the little-finger loops distally and draw down the oblique ulnar little-finger strings, then pass the thumbs to

* Collected by Mrs. A. E. Hodder.

the ulnar side of the straight little-finger string, and turning them up pick up this string on their backs and return through the little-finger loops. Release little fingers.

Transfer thumb-loops to little fingers.

There is now a cross in the centre of the figure. Pass the thumbs (tips pointing away from you) into the lower triangle and back through the upper triangle, thus picking up the cross on the backs of the thumbs. (This movement may be facilitated by picking up the central cross in the mouth.)

Pass the thumbs into the index loops from the proximal side, navaho them and release indices.

There are four longitudinal strings, of which the two middle ones cross each other. Pass index and middle fingers (tips towards you) through the space nearest them between the lateral arms of the cross, pick up between them the (straight) radial thumb string towards the centre of the figure, and return so that this string is wrapped round the tips of the indices. Release thumbs.

Pass the left thumb proximally into the left index loop, and with its back pick up the two strings which cross the figure obliquely.

Pass the right thumb proximally into the left thumb-loops and draw them out.

Pass the thumbs proximally into the index loops, navaho them and release indices.

The ulnar little-finger string is caught down by a small loop to the right and by the lower segment of the central diamond. Pass the left index finger (tip pointing towards you) through the upper segment of the diamond, and pick up on its hook the ulnar little-finger string between the small loop and the diamond. and return.

Pass the left index, still holding this string on its hook, through the left thumb-loop from the distal side.

Release right little finger, and without drawing tight pass it proximal to the right thumb-loop and pick up from the distal side the loop on the left index. Release left index and extend.

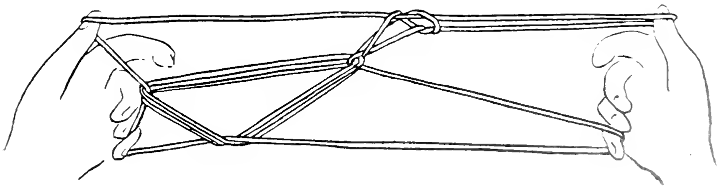


FIG. 14.—THE DUCK.

Release the left little finger, extend gently, and the dead duck is held up by the head.

7. A Man Paddling his Kayak.* (Chukchi, N.E. Asia.)

Position I.

Do the first four movements of "The Duck" (p. 98), except that in the second part of the first movement the left thumb picks up the right ulnar little-finger string from the ulnar side, thus making the figure symmetrical.

Pass each little finger proximally into its respective central diamond, and hook down the strings which cross the thumb-loops obliquely.

Make the thumb-loops common. Pass thumbs proximally into the index loops, navaho them and release indices.

Transfer little-finger loops to indices (the straight string being on the radial side).

Transfer thumb-loops to little fingers (the straight string being on the ulnar side).

Extend figure, and move the index fingers to represent the man paddling his kayak.

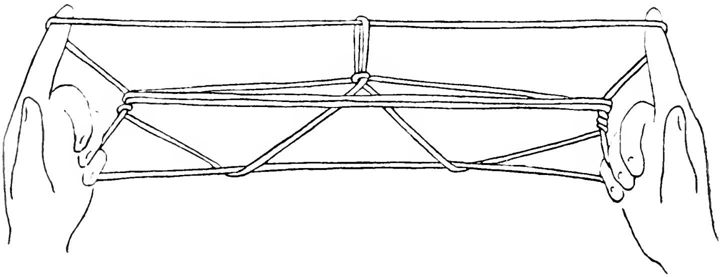


Fig. 15.—A Man Paddling his Kayak.

8. The Eel = *Sirima.* (Mawata, W. Papua.)

Place the string on the little fingers.

Pass the right thumb distal to the strings and pick them up on its back, draw tight. Pass the left thumb proximal to the two right palmar strings, to the ulnar side of the ulnar thumb strings, and draw out.

Pass the thumbs proximal to the two little-finger strings and pick them up. With the index fingers pick up from the proximal side the two ulnar thumb strings.

Raise the right hand and rotate the left hand downwards and towards you, to extend the figure.

* Collected by Mrs. A. E. Hodder.

Another person grasps the middle of the figure in his hand, release thumbs and indices, draw tight, and the eel escapes.

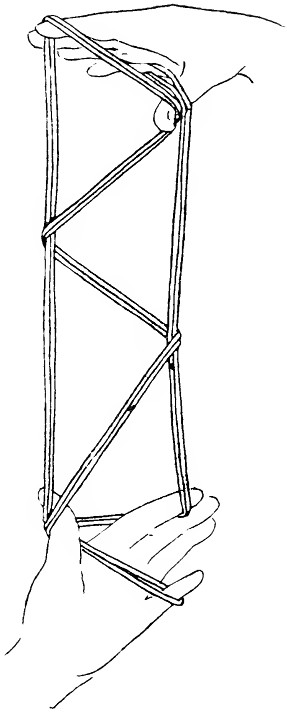


FIG. 16.—THE EEL.

Syn. A thief *ĕnawa* (Baimura, Delta District, Papua). A thief stealing = *henua henua* (Port Moresby, Papua). Also described by Dr. Landtman* as *Sirima* = "a fish." from Kiwai, Mouth of Fly River, Papua.

9. THE TIDE = *Wåru.* (Cape Nelson, N. Papua.)

Opening A.

Release thumbs and pass them proximally into the little-finger loops, pick up the radial little-finger and ulnar thumb strings.

* Landtman, G., "Cat's Cradles of the Kiwai Papuans, British New Guinea," Anthropos ix. 1 and 2, 1914.

Release index fingers and pindiki.*

This represents the high tide which covers the reef.

✳

Release thumbs and pass them proximal to the little-finger loops and into the index loops from the proximal side, turn them down away from you and up towards you, thus picking up the ulnar index strings. Release index fingers.

Pick up the radial little-finger strings with the thumbs, proximally and pindiki. ✳

This represents two large blocks of coral exposed by the falling tide.

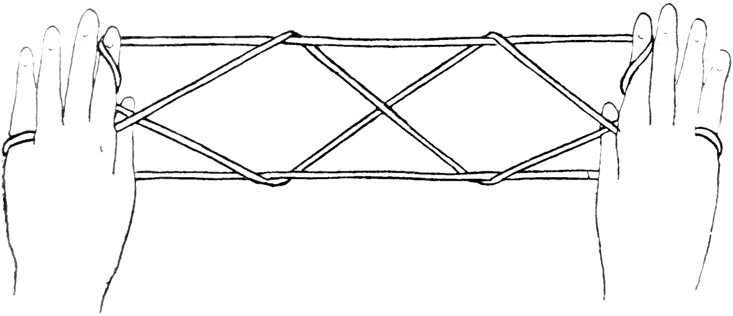


Fig. 17.—The Tide.

Repeat ✳ ✳, and more of the reef appears. Repeat as often as you please until all the reef is exposed.

Release index fingers. Take the two left radial thumb strings in the mouth and release the left hand. Place the left little finger and thumb tip-to-tip with those of the right hand, and slip the loops on to them. Take the mouth strings between the left thumb and index, and turning them over place them on the right thumb and little finger in the opposite direction to that in which they were on the left hand, but making sure that the ulnar little-finger string runs straight across.

Pindiki and the low tide shows all the reef still exposed.

Repeat ✳ ✳, and the rising tide will gradually submerge the reef until it is all covered again.

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* This is a native name for the "Caroline Island Extension." Pass the index fingers proximal to the ulnar thumb string and bring them up through the thumb loop so that this string makes a half-turn round their tips. Keeping the thumbs closely pressed against the index fingers to hold their ulnar strings in place, extend the figure by turning the palms away from you.

This figure is called The Coral Reef = *lagaru* at Mailu, E.C. Papua, and is also known at Mawata, W. Papua. At Mebu Island, at the mouth of the Fly River, it is called "Men going along a path to make a garden" = *gabo* (a path), and shows first the empty path and then two men, and so on; they then return until the path is empty again. In Kiwi Island, near by, it is called "Women going into the bush" = *upi*. Mr. Compton* describes the first part of this figure as "The Sardines" = *Wene ouë*, from Lifu, Loyalty Islands. The four-diamond stage is figured by Roth † as "Four Shrimps," from Princess Charlotte Bay, N. Queensland.

10. An Alaskan River. (Thlinkit, British Columbia.)

Opening A with middle fingers.

Pass thumbs proximal to the middle-finger loops, and with their backs pick up the ulnar middle-finger strings. Release middle fingers.

Transfer little-finger loops to thumbs, keeping these three loops on the thumbs in their right order.

Pass the little fingers into the thumb loops from the proximal side, and hook down the distal ulnar thumb string.

With the right index pick up from the proximal side the distal radial right-thumb string, and with the left index pick up the same (not distal) string from the left thumb, lifting this string off the thumbs.

Lift off the thumbs the straight string passing between them and extend.

This represents a river flowing between two mountains.

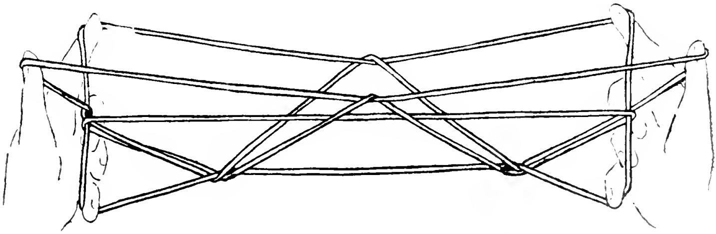

Fig. 18.—An Alaskan River.

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* Compton, R. H., "String Figures from New Caledonia and the Loyalty Islands," Journal of the Royal Anthropological Institute of Great Britain and Ireland, vol. xlix. July to December 1919.

† Roth, W. E., "North Queensland Ethnography," Bul. No. 4, March 1902, plate viii. 2. Copied in Jayne, "String Figures" (see note †, page 106).

Pass the middle fingers proximally through the index loops and take hold of the radial thumb string between the index and middle fingers; turn these fingers down and then up away from you, thus picking up this string on the tips of the index fingers. Release thumbs and extend.

This represents the same river flowing beside a range of mountains.

With the backs of the thumbs pick up from the proximal side the strings which obliquely cross the index-finger loops.

The figure is now in two planes, and represents the mosquito that lives in the river. The proboscis lies in the centre between the thumb and index strings, whilst the long wings sweep down towards the little fingers.

Release index fingers. Pass index fingers into the thumb loops from the distal side, releasing thumb, and extend.

Here the river has reached the plains and has only one mountain beside it.

With the left thumb pick up from the proximal side the straight string which forms the river at the base of the mountain, rotate the thumb once clockwise, thus wrapping the string once round it. Pass the right thumb into the left thumb loops and extend.

Pass thumbs proximally into index loops, navaho them and release index fingers. Transfer thumb loops to index fingers and extend.

This shows a man standing up in a boat, whilst near the right hand is a salmon. (Fig. 18*a*.)

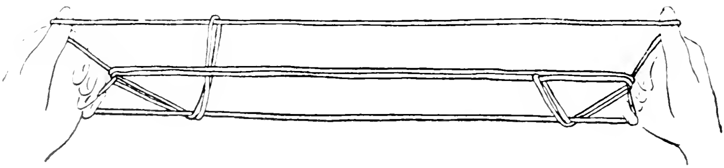

FIG. 18A.—A MAN FISHING AND A SALMON.

Release right little finger; the man throws out his line and catches the salmon.

The first stage of this figure has already been described,* from St. Michael Island, where it is called "Two Mountains and a Stream," = *tituchtak*.

* Gordon, G. B., "Notes on the Western Eskimo." Trans. of the Department of Archæology, Free Museum of Science and Art, Univ. of Pennsylvania, vol. ii. pt. 1, 1906, p. 87.

11. The Looper Caterpillar = *Kultur*. (Yam Island, Torres Straits.)

Position I.

Wrap the radial string once round the left thumb and draw tight.

Pass the right index proximally into the left thumb loop and draw out. Pick up the right palmar string with the left index proximally through the right index loop, and the left palmar string with the right index proximally through the left index loop.

Release the left hand and turn the right hand palm downwards. Pass the left little finger to the back of the right hand and distally into the distal dorsal right index loop and draw it out a little way. Pass the left thumb through this loop proximally and into the proximal right index loop from the *proximal* side, pick this loop up on its back and draw it out through the left little-finger loop. Release right index and return.

Pass the index fingers distally into the thumb loops and draw out the ulnar strings till they are over the little-finger loops, pass the index fingers distally into the little finger loops, and turning them up towards you pick up the radial little-finger strings. Release thumbs, but do not draw tight.

Pass the thumbs distally through the loops just released and pick up the upper string (the original ulnar thumb string) and the ulnar index string.

Release index fingers and pindiki.*

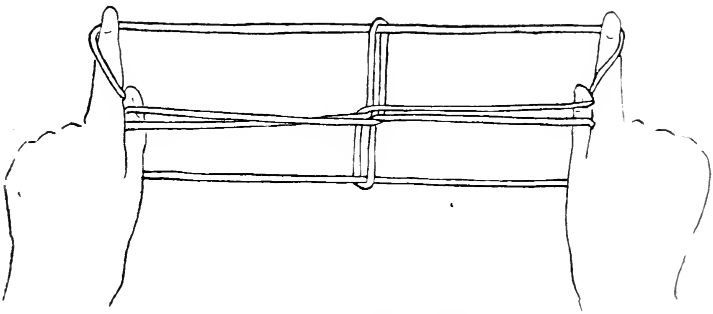

Fig. 19.—The Looper Caterpillar.

Rest the figure on your knee and turn the hands palm upwards, thus causing the "caterpillar" to contract, turn the palms down again and he elongates; repeat making it walk down the leg.

Syn. The Leech = *mānu*. (Saibai, Torres Straits.)

* See note p. 102.

This figure, save for a slight variation in construction, is identical with one collected by Mr. R. H. Compton.* in New Caledonia; it is also known in Lifu. The finished figure is the same as "One Chief" described by Mr. Jayne,† from the Caroline Islands, and as "The Giant Crane" from the Lower Tully River, N. Queensland, figured by Roth.‡

12. THE SCRUB HEN = *Etanga*. (Cape York, N. Queensland.)

Position I. on left hand.

Pass the right hand between the two pendant strings, and separate the thumb and the little finger widely, thus picking up on them the left thumb and little-finger strings respectively, and draw out.

Release the left hand and repeat this movement with it.

Pass the thumbs proximal to the radial little-finger string and pick it up.

Pass the little fingers proximal to the ulnar-thumb string and pick it up.

Do Opening A with single-palmar strings.

Pass the radial-thumb string distal to the rest, and place it over the toe. Slip thumbs out of this string.

Release little fingers, transfer the index loops to the thumbs, and place this double loop over the hands in Position I.

Move hands towards and away from you to imitate the bird scraping together its mound.

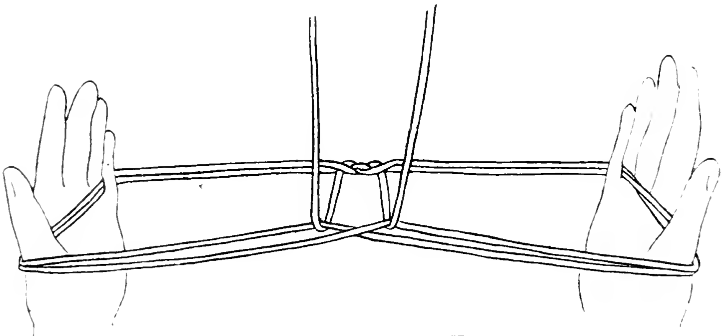

FIG. 20.—THE SCRUB HEN.

* Loc. cit., No. XVIII.

† "String Figures," Pub. Chas. Scribner's Sons, New York, 1906, p. 253.

‡ Loc. cit., plate v. 5.

Take the loop off the hands and rotate through 180 degrees clockwise, and replace in Position I. so that the ulnar little-finger strings are still ulnar, but lie over instead of under the toe strings.

Again imitate the bird scratching.

Release thumbs and lay figure down with the little-finger strings nearest you.

Pick up on the indices their respective toe strings in the centre of the figure and draw out.

This represents the two eggs laid by the Scrub Hen.

Syn. Making Sago = *Dō*. (Kiwi, Mouth of Fly River, Papua.) The sago is kneaded with the hand, turned over and kneaded again, and finally formed into two balls.

13. The Frog = *Parara*. (Kiwi, Mouth of Fly River, Papua.)

Opening A.

Place the ulnar little-finger string over the toe.

Pass the index fingers away from you distal to all the strings, and catch the string running from little finger to toe on the hook of each index and turning it up towards you, return proximal to the radial index strings, allowing these to slip off.

The ulnar thumb strings form a cross in the centre of the figure : take hold of this cross, pull it out, and pass it downwards through the toe loop, release toe and place this double loop over the toe.

Exchange index loops, passing the left through the right.

Bend middle fingers down through the index-finger loops and pick up the ulnar thumb strings proximally on their backs. Release thumbs and indices.

Pass the thumbs proximally into the little-finger loops and draw towards you the radial little-finger string, then pass them distally into the middle-finger loops, hook down the radial middle-finger string, allowing the original radial little-finger string to slip off. Release middle and little fingers, and place the thumb loops on the hands in Position I.

The strings running from the little finger to toe are caught down by a double loop ; pass the index fingers distal to this loop and hook up the upper double string, then bend each index to the palmar side of the palmar string and take this on its back, allowing the double loop to slip off. Release little fingers.

Hook down the ulnar index strings with the little fingers, release indices and extend.

This represents a frog squatting down; holding all the strings taut, release the thumbs and the frog hops off.

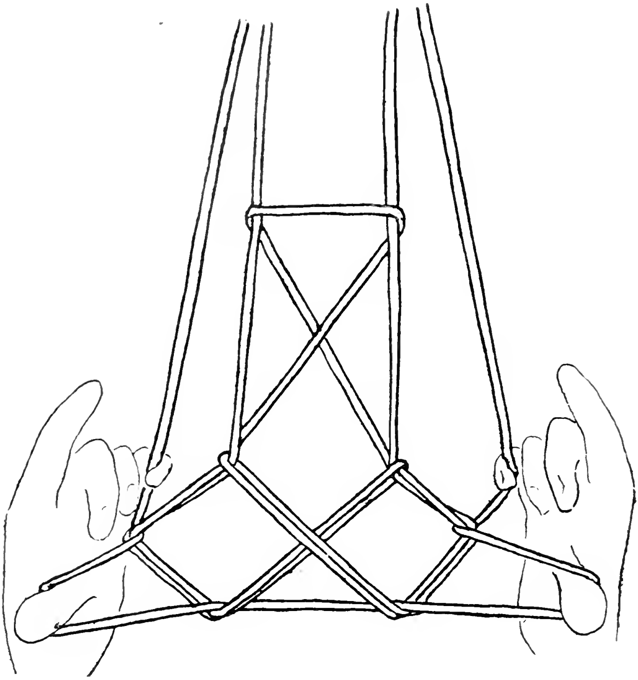

FIG. 21.—THE FROG.

The identical figure is called *Kāāt* in Saibai Island, Torres Straits; *gorogoro* at Maipani, Mouth of Bainu River, W. Papua; and *karakara* at Cape York, N. Queensland—each of which signifies "frog." The Bugi people in W. Papua, however, call it a "Man spearing Dugong from a Platform."

14. "PORKER." (Lifu, Loyalty Islands.)

Hold part of the string between the thumbs and indices, the hands being about six inches apart; make a small loop by bringing the right hand towards you and to the left. Hold the loop between the thumbs and indices so that both the loops hang down, and pass the indices towards you through both loops. Separate the hands by turning up the indices.

Pass the thumbs distal to the proximal radial index string and pick up from the proximal side the proximal ulnar index string.
Pass the thumbs distal to the distal radial index string and pick up from the proximal side the distant ulnar index string.
Pass the little fingers distal to the distal radial index string and pick up from the proximal side the proximal radial index string.
Each little finger is now in a triangle ; pass the index fingers into this triangle from the distal side, and turning them up towards you pick up on their tips the oblique string (i.e the distal radial index string). Release thumbs.
There is now a W-shaped figure between the hands ; with the backs of the thumbs pick up its outer arms. Release indices (three loops from each), and extend.
Pass indices proximally (from the ulnar side) into the thumb-loops and take up the ulnar thumb strings ; lift this loop off the thumbs.
Pass the thumbs proximal to the index loops and distal to the ulnar little-finger string, turn them down and up towards you, thus picking up on their backs the ulnar little-finger string.
Pass the right thumb and index distally through the left index loop, lift the loop off the left thumb, bring it up through the left index loop, and replace it on the thumb without twisting it. Repeat with the little-finger loop. Take off the left index loop and put it over the whole left hand, saying " Down she go." Repeat with the corresponding right-hand loops.
There are now loops on each wrist, thumb and little finger. With the right thumb and index take hold of the left ulnar thumb and radial little-finger strings ; remove the left hand from the figure. With the left thumb and index take hold of these two strings where they were held by the right thumb and index. also grasp the corresponding right ulnar thumb and radial little-finger strings and remove the right hand.
There are now four loops held by the left thumb and index—two original thumb loops (which should be on the radial side of the thumb), and two original little-finger loops (which should be on the ulnar side of the index finger).
Pass the right thumb and index between the two original thumb loops and separating the two digits pick up on them the two loops. Take the two remaining loops held between the left thumb and index in the right thumb and index and remove the left hand.
Pass the left thumb and index between the two original little-finger loops, and separating the two digits pick up on them the two loops. Extend and pull tight.
Hold the right ulnar thumb and radial little-finger strings in the mouth and release right hand. Pass the right fingers towards you horizontally through the two pendant mouth strings and release mouth.

By gently pulling the two upper right-hand strings, the "pig" walks towards the right. (Say "Come along. Porker.") By pulling the two lower strings he walks to the left. (Say "Porker him go.")

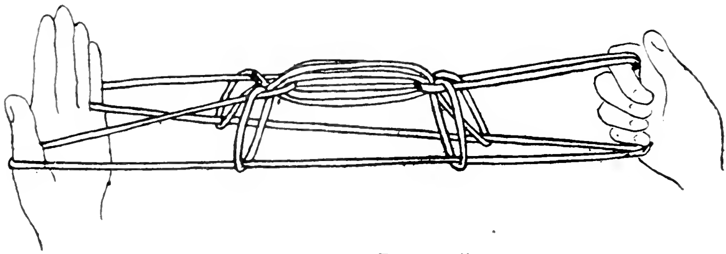

FIG. 22.—"PORKER."

The figure was collected by Mr. R. H. Compton,* who found it was also known to a man from Uvea, Loyalty Islands.

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* Loc. cit., No. XIX.

WEEKLY EVENING MEETING,

Friday, March 19, 1920.

COLONEL E. H. HILLS, C.M.G. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

EDWARD MCCURDY, M.A.

Leonardo da Vinci.

AMONG the greater names in the history of Italian art some are found to be pivotal by reason of the influence of their work upon that of other artists. Giotto and Masaccio are the most conspicuous instances among the earlier masters. In refinement and grace and that mysterious element of symbolism which causes the imagination to be led captive from the seen to the unseen, Giotto is manifestly inferior to certain of the Sienese and such Florentines as Orcagna, who represents the semi-Byzantine succession. His greatness consists in his naturalness and realism. He was the first to discover that, as Ruskin has said, the Holy Family was "just Papa, Mamma, and the Baby." Having discerned this, he set himself to represent the attitudes which persons so related would naturally occupy one to another, and consequently in the inter-relation of characters on the basis of natural association and in study of structure necessary to give due effect to this inter-relation he created the scientific basis of the naturalism of the art of the Renaissance by contrast with the decorative symbolism of the earlier art of Byzantium.

Masaccio reinforced these tenets with noteworthy access of realism in the frescoes in the Church of the Carmine in Florence. In the figures of Adam and Eve in the Expulsion from Paradise every detail is in entire harmony with the dramatic impulse of the motive. Masaccio's work served as an example to all later Florentine art, and the greatest masters of the Quattrocento and Cinquecento were all students of his method.

The names of Antonio Pollainolo and Andrea Verrocchio serve to indicate how in Florentine art of the Quattrocento the study of structure gained new scientific precision from anatomical research.

Piero de' Franceschi reveals a deeper knowledge of the various problems of perspective, arrangement, and light and shade in his works at Arezzo than was possessed by any of his contemporaries. but the influence which his work would naturally exert was restricted by reason of its remoteness from the greater centres of art training. The divergent aims of this small band, who may be termed the

upholders of the scientific tradition in Italian art, are realised with singular completeness in the work of Leonardo da Vinci.

Born in the year 1452, the illegitimate son of a Florentine notary, descended from a long line of Florentine notaries, having shown, according to Vasari, marvellous talent as a boy in the art of design, he was placed by his father in the studio of Andrea Verrocchio, who is described by the same writer as at once goldsmith, master of perspective, sculptor, inlayer of woods, painter and musician. It was apparently a sort of clearing house for ideas for the art world of Florence, and there Leonardo became acquainted with Botticelli and Perugino.

His apprenticeship had ceased in 1472, for in that year his name occurs in the Red Book of the Guild of Painters of Florence.

Records tell of two commissions for altar-pieces in the years 1478 and 1480 respectively. The subject for the first was the Virgin appearing to Saint Bernard. But apparently it never proceeded beyond the stage of preliminary studies. For the second he commenced the large Adoration of the Magi, now in the Uffizi, which although executed only in ground colour reveals such knowledge of perspective and space-composition, together with truth and subtilty of modelling, as shows his genius already in comparative maturity.

The only other works now in existence assigned with certainty to this first period of Leonardo's activity at Florence are the small Annunciation in the Louvre, and the Saint Jerome in the Gallery of the Vatican. The first of these—undoubtedly the artist's earliest extant work—a little panel five inches high by two feet wide, has a suggestion of timidity in the drawing, but yet united with such delicacy and grace as robs timidity of its reproach. The folds of the drapery are simple yet inevitable, fulfilling Leonardo's words in the Treatise on Painting that "drapery should be let fall simply where it is its nature to flow."

The small panel of Saint Jerome, executed only in ground colour, would seem from stylistic considerations to be closely associated in date with the unfinished "Adoration of the Magi."

The modelling of the emaciated figure of the saint is carried to an intensity almost painful to contemplate. There seems already a premonition of the time when the zeal of the scientist should invade the harmony of the artistic conception.

In the year 1483 Leonardo, being then in his thirty-second year, left Florence and went to Milan, where he entered the service of Lodovico Sforza.

I have enumerated all the existing paintings assigned to this first Florentine period, as to the authenticity of which there is unanimity among critics. Add also the half-dozen works mentioned by Vasari, all trace of which has disappeared, but which may be conjecturally assigned to this early period, together with those works in sculpture which Vasari mentions as having been executed by Leonardo in his

youth, and which are naturally to be connected with the period of his apprenticeship to Verrocchio. Leonardo, in a passage in the "Trattato della Pittura," claims to have practised the art of sculpture no less than that of painting, and to do "both the one and the other in the same degree." Making all possible allowance for what may have been lost, the sum total of his complete work in art is yet astonishingly small as covering this period from his apprenticeship to his thirty-second year.

Already in his few pictures the detailed treatment of the herbage, the gradation of the light, the presentment of muscle and tendon, all reveal the scientific study of the laws which defined their structure. The inference is irresistible that already while at Florence he had commenced those studies of natural and applied science the rumour of which, superimposed upon the fame of his artistic work, caused his name to be endowed among his contemporaries with a half legendary universality. Some of the forms of this nascent activity are enumerated by Vasari. I quote from the translation by Mr. Herbert Horne :—

"In architecture he made many drawings, both of plans as of other projections of buildings ; and he was the first, although a mere youth, that put forward the project of reducing the river Arno to a navigable channel, from Pisa to Florence. He made designs for flour-mills, fulling-mills, and machines which might be driven by the force of water. . . .

"And he was for ever making models and designs to enable men to remove mountains with facility, and to bore them in order to pass from one level to another ; and by means of levers, and cranes, and screws, he showed how great weights could be lifted and drawn ; together with methods of emptying harbours, and pumps for drawing up water from low places, all which his brain never ceased from inventing."

According to the statement by one of his earliest biographers, the Anonimo Gaddiano, he was famous as a player of the lyre, and was the instructor of Atalante Migliorotte, who accompanied him when, at the age of thirty, he was sent to the Duke of Milan, bearing a lyre as a present from Lorenzo de' Medici. Vasari, who had access to the MSS. of the Anonimo Gaddiano, mentions his having been sent to Milan with a lyre of silver in the form of a horse's skull, made with his own hands, but places the event twelve years later, when after the death of his nephew, Gian Galeazzo, Ludovic had become by natural succession reigning Duke of Milan. The story of the lyre —common to both narratives—may be accepted as affording proof of his proficiency in yet another of the arts.

In the famous draft of a letter to Ludovic Sforza, in the Codice Atlantico, written presumably immediately on his arrival in Milan, Leonardo offers his services in the capacity of military or naval engineer, detailing the various inventions of which he possesses the

secret, and offering to make trial of any, either in the Ducal Park or in whatsoever place might please his Excellency, in case any of the said inventions should seem to be impossible. In case that natural incredulity, which the writer of the letter apparently expected to meet with, by reason of the scope and variety of the inventions, which comprise pontoons, scaling ladders, cannon or bombards, mines, covered chariots, catapults, mangonels and smoke powders, should dispose any to look on the list as a piece of rodomontade, it may be observed that the contents of Leonardo's manuscripts at Paris and Milan fully substantiate every claim contained in the letter.

The position which Leonardo desired to occupy under Ludovic Sforza was not very unlike that of military engineer and inspector of fortresses which he occupied at a later period in the service of Cæsar Borgia.

The concluding paragraphs of the letter to Ludovic Sforza refer to Leonardo's readiness to be employed in the arts of peace—in architecture as a designer both of public and private buildings, in the construction of water-courses, in painting and in sculpture, whether of marble, bronze or clay, and especially in the execution of the equestrian statue of Francesco Sforza. This project had been formed some years previously by Galeazzo Maria Sforza as a means of commemorating his house, but afterwards set aside through his inability to obtain a sculptor capable of executing it. It was upon this equestrian statue that Leonardo commenced work in the service of the Sforzas. He laboured upon it intermittently for sixteen years. The extent and fervency of the researches that he considered necessary, which comprised studies of various antique equestrian statues, and numerous notes of the proportions of particular horses, as well as a treatise on the anatomy of the horse, brought about that, as was the case with others of his commissions, as Petrarch says, "the work was retarded by desire." Six years after its commencement the Florentine ambassador in Milan wrote to Lorenzo de' Medici, on behalf of Ludovic Sforza, asking him to send an artist capable of executing the statue, because, "although Ludovic has entrusted the work to Leonardo da Vinci, he is not fully persuaded that this master knows how to execute it."

Perhaps Leonardo's creative power was stimulated by the prospect of being superseded. A note in one of his manuscripts states: "On the 23rd day of April, 1490, I began this book and began the horse again." Progress was then fairly continuous. The clay model was exhibited in November, 1493, on the occasion of the marriage of Bianca Maria Sforza with the Emperor Maximilian. The process of casting in bronze would require, Leonardo calculated, a hundred thousand pounds weight of metal. The financial embarrassments of Ludovic's later years no doubt rendered this an impossibility.

The monk Fra Salba da Castiglione, who was present when the French entered Milan in 1499, records the fact of the destruction of the clay model under the arrows of the Gascon bowmen. The statue

ranked with Donatello's Gottamelata at Padua and Verrocchio's Bartolemmo Colleone at Venice as the three great examples of equestrian statues of the Italian Renaissance.

So far as it is possible to form an opinion from the very numerous studies in the Royal Collection at Windsor, it would seem to have been in advance of both the others in freedom and vigour of movement.

The sequence of studies shows a change of purpose from the attitude of the horse galloping to that of it walking. He says in a note in one of his manuscripts, "the trot is almost the nature of the free horse."

The paintings executed during his stay in Milan seem to have been very few in number. Two portraits of Ludovic Sforza's mistresses figure in contemporary record. Either these have perished or they can no longer be identified with certainty. A Nativity painted at the request of the Duke immediately after his arrival in Milan was, according to Vasari, sent as a present to the Emperor. Vasari derives the statement from the book of Antonio Billi, where however the picture is only stated to be an altar-piece.

This may conceivably be a reference to the version of the Virgin of the Rocks now in the Louvre, which on stylistic grounds must be considered to have been painted immediately after Leonardo's arrival in Milan.

The version now in the National Gallery was executed about a dozen years later for the church of S. Francesco at Milan by Leonardo jointly with the Milanese painter Ambrogio de Predis, the association of the two painters in this commission being established by documents recently discovered in the Milanese Archives.

The fact that Leonardo is only responsible in part at most for the actual execution of this picture lessens considerably its value as affording an index in comparison with the earlier version of the change which his art had undergone while at Milan. The picture in the Louvre, however, is entirely Florentine in spirit, and pre-eminent in beauty of line, while that in the National Gallery is Milanese in superior smoothness of texture and chiaroscuro, this latter being one of the distinguishing characteristics of Milanese art, even before Leonardo came to reinforce it with the fruits of scientific study.

The only other painting now in existence the execution of which can be connected with Leonardo's first period of residence in Milan is the haunting ruin of the Last Supper.

The paucity of the list, even allowing for the inevitable mischances of time, confirms the testimony of Fra Sabba da Castiglione, who says that beside the Last Supper few other works in painting by Leonardo were to be seen at Milan in the middle of the sixteenth century, "because when he ought to have attended to painting, in which without doubt he would have proved a new Apelles, he gave himself entirely to geometry, architecture and anatomy."

The external history of his life is sharply divided by circumstances

into three periods. First the early years at Florence. Then his life at Milan under Ludovic Sforza. The third period, that of the Odyssey of wanderings commenced on his leaving Milan with Fra Luca Paciolo two months after the flight of Ludovic Sforza, and extended for the remaining twenty years of his life.

The two proceeded to Venice by way of Mantua, where Leonardo made the sketch of Isabella d'Este now in the Louvre, and undertook to paint her portrait, which promise not being fulfilled occasioned a correspondence—pathetically one-sided—extending over several years.

At Venice, as his manuscripts show, he studied the tides of the Adriatic, and apparently prepared a scheme for flooding part of the Veneto in order to stem the Turkish invasion, and also an apparatus by which it would be possible to approach the Turkish galleys under water.

In the spring of 1500 he went to Florence, where Filippino Lippi resigned to him a commission for an altar-piece for the church of the Annunziata. He chose as subject the Madonna with St. Anne, but only made a cartoon identical in subject with the much later picture in the Louvre.

A note in the Codice Atlantico tells of his hurried departure from Florence to travel in the Romagna as architect and military engineer in the service of Cæsar Borgia. His manuscripts refer to works planned at Urbino, Cesena and Porto Cesenatico. But the office ended with the rebellion of the Duchy, and in March, 1503, Leonardo was once more back in Florence. There he was employed to divert the channel of the Arno, in connection with the war with Pisa. He painted at this time the portrait of Madonna Lisa del Giocondo, the world-famous Mona Lisa, and was also given a commission by the Signoria to paint one of the walls of the Sala del Consiglio. The subject probably chosen for him was the Battle of Anghiari, and the portion executed in colour was known as the Battle for the Standard. So long as the cartoon for this and the subject chosen by Michael Angelo for the opposite wall existed they were, says Benvenuto Cellini. "the school of the world."

He was granted three months' leave of absence by the Signoria at the request of the French Governor of Milan—the three months were afterwards extended, through repeated requests, to sixteen ; and although Leonardo then returned to Florence in connection with litigation over an inheritance he did not resumé work on the battle-piece. He had apparently entered the service of the French. Louis XII. refers to him in a letter to the Signoria as "our painter and engineer in ordinary." He consulted him as to the conduit in the garden of the château of Blois, and employed him on hydraulic work in Lombardy. It was probably in May, 1509. when Louis XII. made a triumphal entry into Milan after the victory of Agnadello, that Leonardo constructed as part of the pageant an automatic lion which walked a few paces and then opening its breast revealed it full of lilies.

There was much study of anatomy with Marc Antonio della Torre at this period, and his intercourse with French artists is shown by a note to enquire from Jean de Paris the method of painting in tempera, but he did not engage in any great artistic work.

In the year 1512 the French lost Milan, and after the re-entry of the Sforzas in the person of the young Maximilian, there is no record of Leonardo's further employment.

On September 24 in the following year, as a note in one of his manuscripts states, he set out from Milan to Rome with his assistants and was there lodged in the Belvedere of the Vatican. Giovanni de' Medici, youngest son of Lorenzo, had been elected to the Papal throne in the earlier part of the year, but whatever hope of employment Leonardo may have cherished was soon frustrated. According to Vasari, the Pope gave him a commission and then was indignant because he began by experimenting with the varnish. The practice of painting, however, had no more than a secondary interest for him. His manuscripts reveal him as engaged in studies in optics, acoustics and geometry, studying geology in the Campagna, improving the method of coining at the Mint at Rome, busy with engineering work at Civita Vecchia, and in studying anatomy at the hospital, for which last-named pursuit he was denounced to the Pope by one of his apprentices.

He seems to have gone with the Papal army to Bologna, where in December, 1515, the Concordat was held between the Pope and Francis I., and a month later he accompanied the king on his return to France with the office of "his painter and engineer," being given as a residence the château of Cloux, near Amboise, where he died on the 2nd of May, 1519.

A record of a visit paid to him at Cloux by the Cardinal of Aragon on the 10th of October, 1517, in a journal kept by the Cardinal's secretary, tells how Leonardo showed them three of his pictures, the portrait of a Florentine lady painted for Giuliano de' Medici, a St. John the Baptist as a youth, and a Madonna and Child in the lap of St. Anne, and how he was then suffering from paralysis of the right hand, but could still make drawings and teach others. The visitors seem to have been particularly impressed by the anatomical drawings, and Leonardo told them that in preparation for these he had dissected more than thirty bodies. They saw also his treatise on the nature of water, and others on various machines, there being as it appeared "an endless number of volumes, all in the vulgar tongue, which if they be published will be profitable and very delectable."

The activities of his mind fall naturally into such as found expression either mainly or in part in constructive work and those revealed only in his writings. The first category comprises painting, sculpture, architecture and engineering. In painting it is enough to instance the fresco of the Last Supper and the portrait of Mona Lisa, each of its type unique among all works of the Renaissance and

beyond all power to appraise in its union of technical mastery and the inevitability of supreme art. In sculpture the Sforza statue, the master work of his Milanese years, lives only in the drawings which furnish some faint index of its power. In architecture there is no outstanding work that survives. Architecture, however, was one of those arts in which he offered his services to Ludovic Sforza. "I believe," he wrote, "I can give you as complete satisfaction as anyone else in the construction of buildings both public and private"; and Sabba da Castiglione speaks of him as having, while at Milan, surrendered himself entirely to the study of geometry, architecture and anatomy. The architectural studies among his manuscripts consist of designs for buildings or monuments, and fragments of treatises, the longest being concerned with fissures or cracks in walls and the nature of arches.

His name occurs in a consultative capacity in connection with that of Bramante, in reference to work in the cathedrals of Milan and Pavia, and he apparently prepared plans for a cupola which was intended to crown the transept of the cathedral. His manuscripts contain many drawings and ground plans for churches, some on the plan of a Latin, some of a Greek cross, with chapels crowned by cupolas grouped round a central dome, and plans for theatres for open-air preaching, with notes of the acoustical properties of the building. There are also architectural drawings of villas, palaces, castles and fortified towers. Others treat of town-planning, and have an elaborate double system of high-level and low-level roadways for different kinds of traffic. The most impressive of his architectural drawings is a design for a large mausoleum in the form of a gigantic cone crowned by a temple. A terrace is cut at two-thirds the height, in which six doorways form the entrances to galleries, each leading to three sepulchral halls. From two opposite sides a flight of steps ascends to the terrace and on to the temple above. The design, which would seem to be a mere fantasy, is characterised by M. de Geymüller as one of the most magnificent in the history of architecture.

Sir Theodore Cook, in his elaborate study of spiral forms entitled "The Curves of Life," has collected a remarkable array of evidence in favour of attributing to Leonardo the design for the open spiral staircase in the Château of Blois. The documentary evidence is missing. But the date of construction is known to have been between the years 1516 and 1519, and Leonardo was then living a few miles distant in the Manor-house of Cloux, near Amboise. A spiral staircase occurs in one of Leonardo's drawings for a fortified tower, and he made many studies of spiral formations occurring in nature, in shells, in smoke, and in the eddies of water.

The staircase at Blois is apparently modelled on the *Voluta vespertilio*, a shell common on the coast of Northern Italy. The theory has obvious attractions. It supplies an example of a work in architecture emanating from the brain of Leonardo, and this a work

of supreme distinction. The title [illegible] one of those conferred upon Leonardo [illegible] remark of the King, [illegible] mentioned specifically as a great architect. The [illegible] reasonable one that he had furnished the King [illegible]. Records of his activity as an engineer [illegible] of canalisation in Florence, in connection with the diversion of the Arno from Pisa as a war measure; in Friuli, under similar circumstances, he devised movable sluices in order to prevent the passage of the Turks across the Isonzo. He made canals in Lombardy for the purpose of irrigation, and also suggestions to improve the water supply of Milan; and the canal of Romorantin, for which he made plans when in France, was intended to connect the waters of the Loire and the Saone.

The potential list of his activities in the construction of instruments of warfare figures in the letter to Ludovic Sforza. He says there:—"I can make armoured wagons safe and immune from attack which will open up a passage through the enemy with their artillery, and however great the multitude of the enemy may be they will be able to break through. And behind them the infantry will be able to follow quite unhurt and without hindrance."

This armoured wagon is seen ready for action in a drawing in the British Museum. It is moved on wheels, and a sketch of the lower half shows the internal machinery, but it is not possible to discern the nature of the motive power. The use of the armoured wagon in order to open up a passage through the enemy, as described above, is identical with that of the tank in the late war.

The manuscripts reveal a strangely prophetic insight in regard to two other developments of recent warfare—namely, poison gas and submarining.

He contemplated the use of poisonous gas or powders in naval warfare for the purpose of suffocating the enemy, and told how to make a simple preventive mask. He also contemplated the contingency—as happened on occasions in Flanders—of an adverse wind causing the poison to recoil upon the users.

The passage, which occurs in the MS. B of the Paris manuscripts, is entitled, "How to throw poison in the form of powder upon ships."

"By means of catapults," he says, "a mixture of powdered quicklime, arsenic, and verdigris may be thrown upon the ships of the enemy, and all who inhale the powder will die.

"But take care that the wind is favourable, lest it blow the powder back upon you, and be sure you have a fine piece of [illegible] to cover the nose and mouth in order that the powder may not enter."

In the second passage, in the Leicester manuscript, he foretells the horrors of submarine warfare, and refuses to impart any information as to the machine which he has constructed lest it should serve to bring them about:—

"How by means of a certain machine many people may stay some time under water. How and why I do not describe my method of remaining under water, or how long I can remain without eating; and I do not publish or divulge this because of the evil nature of men who would use them as means of destruction at the bottom of the sea by smashing the ships in the keel and sinking them together with the men in them. But I will impart others which are not dangerous, because the mouth of the tube by which you breathe appears above the water supported on leather bottles or corks."

In connection with this passage reference may be made to one in MS. B of the Paris MSS. entitled, "A way of escaping in a tempest or shipwreck at sea," in which he tells how to construct a coat of leather of double thickness which will be capable of being inflated when necessary, and thus of serving as a life-saving jacket in case of emergency.

Senatore Luca Beltrami associates the former of these passages with the Turkish war. Leonardo, as a reference in his manuscript shows, had been employed in the construction of a movable dam which should enable the line of the Isonzo to be flooded in the defence of the Veneto against the Turkish invasion. The reference is to the construction of submarine boats in order to sink the Turkish galleys in the Gulf of Venice "by smashing the ships in the keel and sinking them together with the men in them." Leonardo considers this to be justifiable because it is an act of defence "for the safety of our Italian lands" ("delli nostre parti italiche"); but he will not give any details of the construction of his submarine craft, in which it would be possible to remain under water for four hours, because he is fearful of the evil use to which it might be put in future times.

With the list of war inventions may be numbered his researches in aviation. He pursued this subject for many years. His studies range from the consideration of primary causes of flight of birds and other winged creatures to the invention of a screw propeller and the consideration of its applicability to aerial navigation. He also made an actual attempt. Jerome Cardan, the physician who made a horoscope for Edward VI., in his work *De Subtilitate* refers to an unsuccessful attempt at flight made by Leonardo da Vinci, and adds somewhat drily, "he was a great painter." A sentence on the cover of Leonardo's manuscript, Sul Velo degli Uccelli, written in 1505, has been interpreted as referring to this attempt. "The great bird," it runs, "will take its first flight upon the back of the great swan, filling the whole world with amazement, and filling all records with its fame; and it will bring eternal glory to the nest where it was born."

This enigmatic utterance may be somewhat more comprehensible if it is remembered that *cecero* is the Italian word for swan, and "the back of the great swan" may therefore be interpreted as a reference to Mont Ceceri, a hill to the south-west of Fiesole, from which it is believed the flight took place.

From the meagre records of the attempt we pass to the researches in theory and construction. In an article in the "Nineteenth Century" ten years ago I attempted a fuller analysis of these than is here possible, and I may be permitted to quote a sentence from my article :—

"The material falls naturally into two groups, the first being a series of investigations of the laws which govern the power of flight as manifested in nature by birds and other winged creatures, the second consisting of deductions from these principles in the construction of a mechanism which should be capable of sustaining and being worked by man. The inter-dependence of the two parts of the enquiry is stated with great succinctness in a passage in the Codice Atlantico :—

"'A bird is an instrument working according to mathematical law, which instrument it is within the capacity of man to reproduce with all its movements, but not with a corresponding degree of strength, though it is deficient only in the power of maintaining equilibrium. We may therefore say that such an instrument constructed by man is lacking in nothing except the life of the bird, and this life must needs be supplied from that of man.

"'The life which resides in the bird's members will without doubt better conform to their needs than will that of man which is separated from them, and especially in the almost imperceptible movements which preserve equilibrium.

"'But since we see that the bird is equipped for many obvious varieties of movements, we are able from this experience to deduce that the most rudimentary of these movements will be capable of being comprehended by man's understanding : and that he will to a great extent be able to provide against the destruction of that instrument of which he has himself become the living principle and the propeller.'"

In the analogy thus drawn from nature to the problem before him Leonardo has anticipated the attitude of modern research.

In his construction of the instrument he finally attempted to combine the type of the lark soaring with its wings open with that of the bat as it descends. He does this by the introduction of *sportelli* (trap-doors or shutters) in the surface of the wings, whereby, as he says, "the wing is full of holes as it rises and closes up when it falls." The shutters should have rims of cane and be covered with starched taffeta to render them airtight. Perhaps it was after the Monte Ceceri attempt that he wrote on a page of MS. B of the Paris MSS., "Try the actual instrument in the water, so that if you fall you will not do yourself any harm." It may also have been the failure of this attempt that caused him to search for a fresh source of motive power to take the place of that exerted by the muscles of a man. On 83 verso, MS. B of the Paris MSS., there is a drawing of a large screw constructed to revolve round a vertical axis, and a note explains its intended use : "If this instrument made with a screw

is well made, that is to say made of linen of which the pores are stopped up with starch, and is turned swiftly, the said screw will make its spiral in the air, and it will rise high."

He adds that a small model may be made of cardboard with the axis formed of fine steel wire bent by force, and that this when released will turn the screw. To his drawing of this instrument the architect Luca Beltrami has—to me, as it seems, justly—applied the word aeroplane.

Another page in the Codice Atlantico (311 v. d.) of unique interest contains three studies of artificial wings, a name, and a note that the machine is to be made not with "*sportelli*," that is shutters, but united. The natural interpretation is that the note refers to a commission for the construction of a machine for flight, with regard to which the patron Gian Antonio de Mariolo has expressed a desire that the wings should be such that no wind would be able to pass through them as it would if they had shutters, i.e. should be like the wings of the bat.

I might occupy as many hours as I have minutes at my disposal without in any way exhausting the sum of his researches in natural and applied science. They cover so wide a field, and specialisation in these days has so divided knowledge into water-tight compartments, that to properly gauge the value of his contributions to scientific research would require a combination of many trained intelligences. But it is not possible to devote a number of years to the close study of all that concerns Leonardo without becoming imbued with the conviction of the complete oneness of his work and method. The dominant purpose which animates him, whatever the nature of the problem, is to investigate, to examine, and define primary causes. His pen reinforces his practice. "Nature," he says, "is constrained by the order of her own law, which lives and works within her." Again, "There is no result in nature without a cause; understand the cause and you will have no need of the experiment"; and, "Nature is full of infinite causes which were never set forth in experience."

Maybe the purpose to investigate arose primarily out of what he conceived to be the necessities of his art. "The painter," as he says, "contends with and rivals nature," and as a means to this end he must acquire all possible knowledge of her processes.

With Leonardo the latter end of this search forgot the beginning. His intellectual curiosity into the origins and causes of all created things is revealed in infinite variety in the thousands of pages of his manuscripts, compact, as has been said, "of observation, of prophecy, of achievement," and in this triple legacy forming a record probably unequalled, certainly unsurpassed, by that of any other man in the history of the world. For consider what he was! Printer, painter, sculptor, engineer, architect—all these to the wonder of his contemporaries. His manuscripts reveal that he was no less distin-

guished as physicist, biologist and philosopher. But in the field of science he was essentially a forerunner. The results that he achieved must be reckoned as small compared with his grasp of basic principles, with the vistas that he opened up, and the unerring instinct which he displayed in choosing the true method of investigation.

Science, as he held, came by observation, not by authority, but to say this is not to deny the use of the inherited wisdom of the ancient writers as being the natural starting point of all knowledge, even that founded on observation and experiment. References to the scientific authors of antiquity occur frequently in the manuscripts, and a few memoranda as to the whereabouts of books which he apparently intended to consult may be quoted as showing how this knowledge was built up as occasion offered.

"Enquire at the Stationer's about Vitruvius." and, again, "Messer Ottaviano Palavicino for his Vitruvius."

"The Archimedes which belongs to the Bishop of Padua."

"Maestro Stefano Caponi, a physician, lives at the piscina and has Euclid 'De Ponderibus.'"

"The heirs of Maestro Giovanni Ghiringallo have the works of Pelacano."

"An algebra, which the Morliani have, written by their father."

"Try to get Vitolone which is in the Library at Pavia, and which treats of mathematics."

"A grandson of Gian Angelo the painter has a book on water which belonged to his father."

A list of forty books of a more general character on a page of the Codice Atlantico is generally accepted as indicating a portion of Leonardo's own library; among the titles are an Arithmetic, a Bible, a copy of the Psalms, Pliny, Livy, Ovid's Epistles, Petrarch, and the Travels of John de Mandeville.

References also occur in the manuscripts to Latin poets, Virgil, Horace, Lucretius, but far more numerous are the references to his predecessors in scientific investigation, such as Aristotle and Archimedes. He refers also to Avicenna, Galen, Clemedes, Hippocrates, and Roger Bacon.

The predominantly scientific character of his mind is shown by the fact that the record of his friendships is concerned almost exclusively with his scientific pursuits. In a note in the manuscripts he says, "Vespuccio will give me a book of Geometry," and Vasari says that he made a drawing of the head of Amerigo Vespucci as an old man, this being presumably after his return to Florence in 1502, as Vespucci was born in 1451. The friendship thus indicated must have stimulated that interest in physical geography which led to his making enquiry by letter of the condition of the tides in the Black and Caspian Seas, and may have been instrumental in causing him to construct many maps. A map of the world attributed to him, in the Royal Library at Windsor, is the earliest map on which the name

America is applied to the Western Continent. It is also the earliest on which the severance of the western coast of America from Asia is clearly indicated.

The famous mathematician Fra Luca Paciolo, whose "Summa de Aritmetica Geometrica" Leonardo had acquired at Pavia on its first appearance, came to Milan in 1496, and Leonardo immediately became associated with him in his studies and drew the figures, sixty in number, for his treatise "De Divina Proportione." The two left Milan together two months after the entry of the French, and Paciolo accompanied him to Venice, where he continued to be preoccupied with the study of mathematics.

During his second residence in Milan the chair of anatomy at Pavia was held by Marc Antonio della Torre, and Vasari bears emphatic testimony to the manner in which the two mutually assisted each other in anatomical research. Leonardo's own studies, as his manuscripts show, extended over more than a quarter of a century.

All his writings connected with the subject seem as it were fragments of a larger purpose, charted, defined, explored, but never fulfilled, of which his researches in anatomy, zoology, physiology, embryology and biology are the allied and component parts. Discerning the essential unity of man and the animals, "because," as he says, "all land animals have similar members, that is to say, muscles, nerves and bones, and these members do not vary at all except in length and thickness" (MS. G 5 verso), he may be said to have founded comparative anatomy. Drawings now at Windsor show the gradations of the human type merging into that of various animals. He tracks the mystery of life from the conception and the fœtus through growth to maturity, and so to the gradual wasting of the tendons and all the physical phenomena of death.

"I have dissected," he says, "more than ten human bodies, destroying all the various members, and removing even the very smallest particles of the flesh which surrounded these veins without causing any effusion of blood other than the imperceptible bleeding of the capillary veins. And, as one single body did not suffice for so long a time, it was necessary to proceed by stages with so many bodies as would render my knowledge complete; and this I repeated twice over in order to discover the differences."

The drawings made in the course of these investigations, now in the Royal Collection at Windsor, were examined in the time of George III. by the famous surgeon William Hunter, who, approaching them with all the natural professional mistrust, thus made the *amende honorable* :—

"I expected," he says, "to see little more than such designs in anatomy as might be useful to a painter in his own profession. But I saw, and indeed with astonishment, that Leonardo had been a general and deep student. When I consider what pains he has taken upon every part of the body, the superiority of his universal genius,

his particular excellence in mechanics and hydraulics, and the attention with which such a man would examine and see objects which he has to draw, I am fully persuaded that Leonardo was the best Anatomist at that time in the world." Although he does not fully explain its mechanism, he evidently knew of the circulation of the blood a hundred years before Harvey gave the knowledge to the world. "The heart," he wrote, "is a muscle of great strength; the blood which returns when the heart opens again is not the same as that which closes the valve."

The depth and variety of his researches in other branches of natural science may be inferred from the citation of a few instances in which he anticipated the results of investigations associated with other names. Either before or at latest during such time as Copernicus was laying the foundations of his heliocentric theory by study at Bologna and Padua—a theory afterwards brought to completion and published in his work, "De Revolutionibus Orbium Cœlestium," in 1543—Leonardo had enunciated the ruling principle of it in a line in the manuscripts now at Windsor, "Il sole non si muove" ("The sun does not move").

A hundred years before Maestlin, who is credited with the discovery, he had defined the obscure light of the unilluminated part of the moon as due to reflection from the earth's surface.

In the search for hidden laws and causes the scientific problem followed hard upon the artistic problem. The study of perspective led to that of light and shade, and so of optics—the study of the structure and functions of the eye—as being the instrument by which light and shade are perceived. He made a model of its parts, and showed how an image is formed on the retina, thus refuting the currently accepted belief of the eye throwing out rays which touch the object it desires to examine. He described also the principle of the *camera obscura* ninety years before Porta developed the idea in practice.

In mechanics he enunciated the theory of inertia, afterwards demonstrated by Galileo, and relegated the theory of perpetual motion then current to the same category as astrology and necromancy. He refound the wisdom of Archimedes, and demonstrated his theory of oblique forces applied to the arm of the lever, afterwards associated with the name of Galileo. Following on Archimedes' conception of the pressure of fluids, he showed—a century and a-half before Pascal—that liquids stand at the same level in communicating vessels, while if the two arms are filled by different liquids the heights will vary inversely as their densities.

He is at once artist and scientist in his treatment of and interest in water. He studies its properties and power of movement under conditions varying from the action of the tides of the ocean to the laws which regulate the movement of water in syphons, a subject on which he notes his intention of writing a treatise. He follows its transformation into vapour, rain, dew, snow and ice. It winds

mysteriously in wonder-working coils through the landscape backgrounds of his pictures. He traces the infinite shapes it assumes, falling in violence of movement in spirals and eddies, circling like the loop of a swallow's flight, something of the artist's sheer delight in the creation of beauty of form mingling with the scientist's purpose to wrest from this variety its underlying principle. Or again, as engineer he harnesses its power, studying to divert its channels either in menace of war or for purposes of commerce or irrigation.

In considering a geological problem his method is entirely deductive. "Since," as he says, "things are far more ancient than letters," he turns from authority to the testimony of things themselves. "Why," he asks, "do we find the bones of great fishes and oysters and corals and various other shells and sea-shells on the high summits of mountains by the sea just as we find them in low seas?"

The fact that the cockles were living at the time when they became embedded in the strata—this being evident from the shells being found in a row in pairs, while in other places the dead are found separated from their shells and all cast up together by the waves—is cited as proof that water formerly covered parts of the earth which are now far above the level of the sea, and that this condition continued for a period of more than the forty days of the Deluge, because, as the cockle travels along a furrow at the rate of three or four braccia daily, it could not in forty days have proceeded from the Adriatic to Monferrato in Lombardy, a distance of 250 miles. By an investigation of the cuttings formed by the Arno in the successive strata of which the shells are found, he shows the gradual changes in the crust of the earth, and following on the track of this knowledge he essays the construction of the map of Italy in days remote beyond record, but of which the earth remains a living witness.

His special interest in botanical study may be traced back to the earliest period of his artistic work.

Vasari tells of a cartoon, intended for tapestry, of the sin of Adam and Eve in Paradise, where was a meadow with innumerable plants and animals "of which in truth one could say that for diligence and truth to nature divine wit could not make the like." He mentions a fig-tree as of special excellence for the foreshortening of the leaves and the disposition of the branches, and also a palm in which the roundness of the fan-like leaves was shown with marvellous art. His description suggests minute attention to detail on the part of the artist based upon a profound study of nature, and these are the characteristics which find expression in Leonardo's many exquisite studies of plants and flowers, and the treatment of the herbage in the Virgin of the Rocks in the Louvre. His study of botany was in inception an integral part of his treatise on painting, botany being as necessary as anatomy, in order that the painter might have the requisite knowledge of form and structure.

But here also the artist's power of observation of the varied

beauty of earth's raiment of plants and flowers is merged imperceptibly in the mood of the scientist who saw in nature not only form and colour, but above all light, which St. Augustine called "the queen of colours," and uses nature's profusion as a background whereon to study the incidence of light and shade.

"When the sun is in the east," he says, "all the parts of trees which are illuminated by it are of a most brilliant green, and this is due to the fact that the leaves illuminated by the sun within half our hemisphere, namely, the eastern half, are transparent; while within the western semicircle the verdure has a sombre hue, and the air is damp and heavy, of the colour of dark ashes, so that it is not transparent like that in the east, which is refulgent, and the more so as it is more full of moisture."

Or this, "Of Landscapes":—

"The dark colours of the shadows of mountains at a great distance take a more beautiful and purer blue than do those parts which are in light, and from this it follows that when the rock of the mountains is reddish, the parts of it which are in light are fawn-coloured, and the more brightly it is illuminated the more closely will it retain its natural colour."

His researches in structure are so exact and so scientific in method as to anticipate the results of subsequent enquiry, as, for instance, in the knowledge his writings reveal of phyllotaxis—the law of quincuncial arrangement of the leaves on the stem—promulgated in 1658 by Sir Thomas Browne in his "Garden of Cyrus."

In like manner the discovery that the age of a tree may be told from the number of concentric rings visible in a section of its trunk, with which more than a century later the names of Nathaniel Grew and Marcello Malpighi are associated, is contained in a passage in Leonardo's "Treatise on Painting" (Ludvig, 829). He also states in the same passage that these rings vary in thickness according to the greater or less amount of humidity of each year.

"In an age of so much dogmatism," wrote Hallam, "it was Leonardo who first laid down the grand principle of Bacon, that experiment and observation must be the guides to just theory in the investigation of nature."

I have attempted here to summarize a few of the results attained in the course of this investigation. The breadth and variety of their scope may serve to recall the remark of Francis I., who is recorded by Benvenuto Cellini to have said "that he did not believe that any other man had come into the world who had attained so great knowledge as Leonardo." The proof of this lies in the thousand pages of his manuscripts. If it does not fully appear in these extracts, I may plead, just as Leonardo does in the concluding words of one of his anatomical manuscripts, "I have not been hindered either by avarice or negligence, but only by want of time."

[E. McC.]

GENERAL MONTHLY MEETING,

Monday, April 12, 1920.

J. H. Balfour Browne, K.C. D.L. J.P. LL.D.,
Vice-President, in the Chair.

F. W. Bain, C.I.E.
Charles James Soutain Hancock, M.D.
Sir Leigh Hoskyns, Bart, J.P.
Mrs. Bayford Owen,
Captain Charles Sampson, R.A.M.C. (T.F.),
Francis C. M. Welles,
Miss Zula Maud Woodhull,

were elected Members.

The Chairman reported the decease of Mr. C. E. Groves, on February 1, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously adopted :—

Resolved, That the Managers desire to record their sense of the great loss sustained by the Institution by the death of Charles Edward Groves, F.R.S. F.C.S. F.I.C. M.R.I.

Mr. Groves was a Member of the Royal Institution for twenty-one years, and always took a keen interest in its welfare. He was elected a Visitor in 1905, and in 1906 was selected by the Committee of Visitors as their Secretary. He held the Visitorial Secretaryship for six years, and sacrificed a large amount of time and attention to the improvement of the structure and maintenance of the internal condition of the building of the Royal Institution. He was a Student under the distinguished Professor Hofmann at the Royal College of Chemistry. He became Senior Assistant to Dr. John Stenhouse, F.R.S., Chemist to the Mint and Lecturer on Chemistry at St. Bartholomew's Hospital in 1882, and remained with him until his death. For five years he took part with Dr. Stenhouse in the work of an external Assayer to the Royal Mint, and was Collaborator in Research with him. He was conjoint Author with Dr. Stenhouse of twelve important Papers in the Scientific Journals, between 1875 and 1881, on the domain of Organic Chemistry, dealing with the Phenols, Naphthalin, Resin and Orcin Derivatives, and Colouring Matter from Lichens. He was appointed in 1882 Lecturer in Practical Chemistry to Guy's Hospital, becoming subsequently Senior Lecturer in Chemistry and Lecturer in Dental Metallurgy; and in 1885, Consulting Chemist to the Thames Conservancy. He was the Editor of the Journal of the Chemical Society from 1884 to 1899, and of several important works, including "Organic Chemistry," in 1880; "Fuel," in 1889; and "Groves' and Thorp's Chemical Technology."

The Managers desire, on behalf of the Members, to express their deep sympathy with his sisters in their bereavement.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz :—

FROM

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WEEKLY EVENING MEETING,

Friday, April 16, 1920.

Sir William Phipson Beale, Bart., K.C. F.C.S. F.E.S., Vice-President, in the Chair.

H. Maxwell Lefroy, M.A. F.E.S. F.Z.S., Professor of Entomology, Imperial College of Science, London.

The Menace of Man's Dispersal of Insect Pests.

[Abstract.]

In the past destructive insects have been carried about the world on plants and in merchandise; to England have come such pests as codlin moth of apple, woolly aphis of apple, hessian fly of wheat,

black currant mite and the white fly of tomato. Elsewhere far more destructive pests have been carried, such pests as Mexican boll weevil, and pink boll worm of cotton, Colorado beetle, gipsy moth, corn moth, fluted scale, Hawaiian cane fly, San José scale and others; these are pests of the very first magnitude whose effects on crops amount to millions sterling. The future menace is greater owing to (1) increased railway communications provided by the Channel tunnel, Gibraltar tunnel, the Cairo-Baghdad link (completed), and presently the Basrah-Karachi, the India-Burma and Burma-China linking up; and (2) the aeroplane, which bridges sea routes at present only covered by boats. The construction of aeroplanes and airships allows them to carry insects: and whereas trains and boats start from stations and docks in towns, remote from field and orchard, planes and airships get up from fields and come down again in fields, thereby directly transmitting the crop pest.

Short cuts bridged by aircraft are:—Africa–Brazil; Bombay–Africa; Peshawar–Turkestan; Calcutta–Rangoon–Bangkok: Japan–China; Philippine Islands–Borneo–India; Cairo–Baghdad–Karachi; Java–Queensland; Italy–Tripoli; Florida–Cuba–Yucatan.

In addition to carrying crop pests the aircraft will make possible the rapid transmission of the tse-tse flies, mosquitoes with yellow fever and malaria, lice with typhus, trench fever and relapsing fever; and this will be used as a war measure in future wars. Not only diseases, but virulent pests of crops, such as the bollworms, cane borers, rubber, tea and coffee pests will be spread from one area to another as an offensive measure.

[H. M. L.]

WEEKLY EVENING MEETING,

Friday, April 23, 1920.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Israel Gollancz.

Shakespeare's "Shylock" and Scott's "Isaac of York."

[No Abstract.]

WEEKLY EVENING MEETING.

Friday, April 30, 1920.

J. H. Balfour Browne, K.C. D.L. J.P. LL.D. Vice-President, in the Chair.

F. O. Bower, Pres.R.S.E. Sc.D. LL.D. F.R.S.,
Regius Professor of Botany, University of Glasgow.

The Earliest Known Land Flora.

The Vegetable Kingdom is made up of plants of most varied size, character and habitat. Comparing those various types, the view becomes ever more insistent that dependence on water is the master-factor determining their existence. As we range their diverse forms according to probable sequences of descent, those which we regard as the most primitive according to their structure and mode of reproduction are those which are habitually the most dependent upon constant water supply. It is the same with the Animal Kingdom. These broad results were summed up by Weismann some forty years ago in the statement that the birth-place of all animal and plant life lay in the sea. If this be true, it follows that all life on exposed land-surfaces has been secondary, and derivative.

Geologists tell us that from the remotest past land-surfaces have stood exposed above the level of the ocean. The continents and islands may have differed from time to time in their outline and area from those of the present day. But we may believe that from the earliest period land-surfaces have had a continuous existence, so that life upon land may itself have been continuous from the time when living organisms first emerged from their natal waters. Such beliefs throw back to the very remote past the possible origin of life upon dry land. But still the probability remains that aquatic life antedated that event. These considerations lead inevitably to the questions: When was dry land first invaded from the water? What were the first land-living plants and animals like? And how did they rank as compared with modern life?

Leaving zoologists to solve these questions for their own branch, we botanists are to-day in a better position than ever before to answer them with regard to plants. Though still far from being able to visualize the beginning of the story, recent discoveries have made it possible to see clearly and in detail the nature of the earliest known land flora, which is that of the Lower Devonian Period. During recent years fossil plants of the Lower Devonian have been

found in Sweden and in Scotland in greater profusion than ever before, while the Scottish specimens are so well preserved that they are now almost as well known in structural detail as plants of the present day. Already in this room repeated lectures have been given on the Palæozoic Flora. Many plants of the Carboniferous Period have been described here in microscopic detail, and they are mostly referable to affinity with such living types as Ferns, Club-Mosses and Horsetails. Some, such as the Sphenophylls and Pteridosperms, represent classes which have since died out. But, speaking generally, the flora of the coal is composed of plants comparable with the lower vascular plants now living. They possessed stems, leaves, roots and sporangia. Some even produced seeds like modern Gymnosperms.

Passing back from the Carboniferous Period to the Upper Devonian, the flora, though more restricted, may still be described in terms applicable to the living vegetation. They include among others the gigantic fern-like plant, *Archæopteris hibernica*, from Kiltorkan, Co. Kilkenny; the large Lycopod *Bothrodendron*, from the same source: and the large-leaved *Pseudobornia*, from Bear Island. Flat leaf-expansions are here seen, and the plants named have been referred in their general characters respectively to affinity with the Ferns, Club-Mosses and Horsetails. But between the Upper Devonian and the Lower, geologists tell us that a vast period of time intervened. The evidence of the plant-remains supports this. The Lower Devonian fossils so far known are meagre in number of forms. In their characters they differ more markedly from the plants of the present day than any of their successors. They were rootless, and there appears to be a complete absence of large flattened leaf-expansions. It is upon them that the new discoveries have shed so interesting a light. Conversely, that light is reflected back by comparison upon the more recent forms. In fact, a new chapter has been opened in plant-morphology, and a new class of vascular plants, the Psilophytales, has been established to receive these representatives of the oldest known land flora. The study of them is leading to new interpretations of the form shown by plants of later periods, and ultimately of the present day.

Until 1913 the plants of the Lower Devonian Rocks were very imperfectly known. Their recognised characters were chiefly negative. There was no evidence of broad leaf-surfaces, nor was it clear whether or not they bore leaves as distinct from stems. The existence of true roots was also doubtful. The best known plants were constructed of approximately cylindrical stalks bearing lateral spines. These stalks arose from a branched and creeping base. Some of them showed crozier-like curves when young, and sporangium-like bodies were sometimes found upon them. The most distinctive of these plants were grouped by Dawson in his genus *Psilophyton*, and he published a drawing of his reconstruction

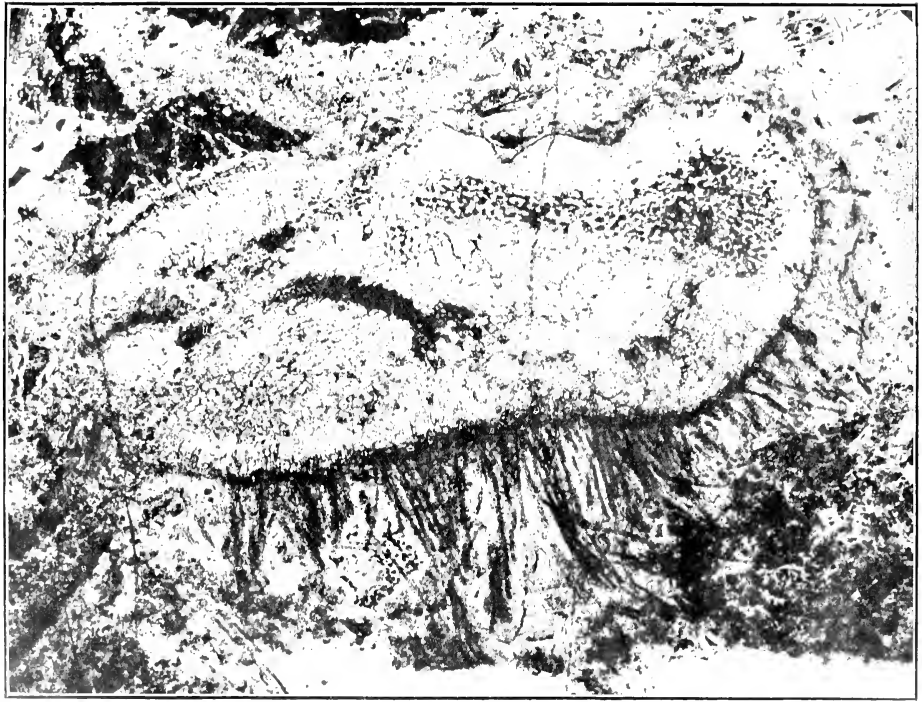

Fig. 1.—Vertical Section through the Protocorm of *Hornea Lignieri* with Rhizoids, embedded in Peat (× 14).

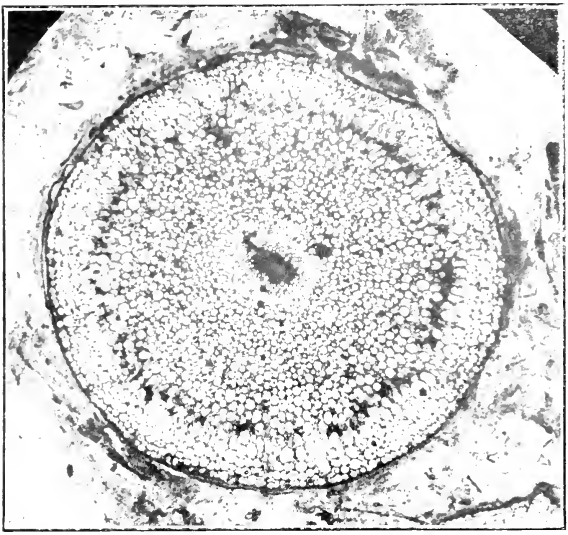

Fig. 2.—*Rhynia major* Aerial Stem seen in Transverse Section (× 20).

Fig. 3.—Stoma of *Asteroxylon Mackiei*, in Surface View (× 210).

of *P. princeps*. It was, however, the subject of adverse criticism by his contemporaries, and the validity of the genus was questioned.

It was upon a field so open as this that light has now been shed. From fresh-water deposits of Lower Devonian age round Lake Röragen, on the frontier between Norway and Sweden, Dr. Halle collected many specimens of fossil plants. But they were mostly impressions, and showed only imperfect preservation of their microscopic structure. He distinguished several genera of plants with branched cylindrical stems bearing small thorn-like appendages, and some of them distal sporangia. Many of his specimens were referred to *Psilophyton princeps*, and bore out in the main the reconstruction of Dawson. Halle was able to confirm the existence of a central vascular strand in *Psilophyton*, consisting of tracheides, a fact which ranks it with certainty among vascular plants of the land. But the most distinctive novelty which Halle discovered in the Röragen beds was a fossil which he called *Sporogonites*. It consisted of a simple stalk bearing a terminal capsule. From its form, and the character of its contents, he held it to be a sporogonium comparable with that of the Bryophytes; but a generalised type, not referable to any existing group of them. An alternative suggestion was that *Sporogonites* may represent only the upper part of a more highly developed sporophyte, perhaps on the line of descent of the Pteridophytes. Thus the presence of *Sporogonites* does not actually prove the existence of Bryophytes as we now know them in the Lower Devonian Rocks. But nevertheless it has a peculiar interest. Hitherto there has been no certain record of the existence of any moss-like type in the Palæozoic Period. The demonstration of so moss-like a sporangium as *Sporogonites* is certainly the most thrilling of the facts brought forward by Dr. Halle.

In 1913, three years before Dr. Halle's publication of these discoveries at Röragen, the first of the new observations of Lower Devonian plants in Scotland was recorded. Dr. Mackie, of Elgin, found at Rhynie, in Aberdeenshire, certain isolated blocks of chert containing plant remains. A little later the source of these blocks was traced to a bed of chert, of Old Red Sandstone age, found *in situ* by the Scottish Geological Survey. Its origin appears to have been this. An exposed land-surface existed there in Lower Devonian time, subject to intervals of inundation. It became periodically covered by vegetation. By decay of its stems and underground parts a bed of peat would be formed. The peat was then flooded, and loose sand deposited over it. Again the vegetation was repeated, and so successive bands were formed to some eight feet in thickness. Then followed water with silica in solution, supplied from some fumarole or geyser. The peat-bed was thus sealed up, and the plants preserved with astonishing perfection.

From this bed of chert four distinct vascular plants have been recognised, and described in the minutest detail by Dr. Kidston and

Professor Lang. They are all essentially similar in type, though sufficiently different to be placed in three genera, named respectively *Rhynia* (two species), *Hornea* and *Asteroxylon*. *Rhynia* and *Hornea* are leafless and rootless, while *Asteroxylon* is also rootless, but it bears leaves of a simple type. The plants thus clearly indicate a primitive state prevalent at that period. They conform in general features to the type of *Psilophyton* as described by Dawson, and as recognised in greater detail by Halle. But here in the Rhynie chert the structural details are so well preserved that these earliest of all known vascular plants can be examined and described almost as well as any modern living plants. Some have even been found standing erect as in life. Through untold ages, like the legendary Knights of the Round Table, they have thus awaited the revivifying touch of Modern Science.

Of the four plants so far described from the Rhynie chert, *Hornea Lignieri* is relatively simple. From a distended and lobed protocormous base rose the stems, which bifurcated. These bore distal sporangia, which represent their transformed tips. Sometimes the sporangia were themselves forked. The protocorm was bedded in the peat, and parenchymatous, with many rhizoids (Fig. 1). The cylindrical stems stood upright from it, and were about 2 mm. in diameter. They were traversed by a simple stele with a solid core of tracheides, surrounded by phlöem. The stele forked at the dichotomies of the stem, but stopped short at the base of the sterile columella, which ran upwards into the flat-topped, and apparently indehiscent, sporangium. The latter appears as a transformation of the end of the stalk, which is simply an ordinary branch of the plant. The spores are tetrahedal, as they are in all of these plants of the chert. The general aspect of *Hornea* is such as to provoke comparison with the Bryophytes, notwithstanding certain strongly divergent characters. This may have some real significance in view of its small size, and relatively simple structure.

Rhynia major is larger and better preserved, but still it also is structurally simple (Fig. 2). It had a less distended rhizome, from which the robust cylindrical stems arose. These consisted, as in *Hornea*, of a central stele with solid xylem-core and investing phlöem, surrounded by a massive cortex, of which the inner region appears to have been photosynthetic. Outside was a well-marked epidermis with stomata (Fig. 3). These and the vascular tissue prove the aerial habit of the plant. The stems ended in solitary massive sporangia, as much as 12 mm. in length, without a columella, and filled with tetrahedal spores (Fig. 4).

Neither of the species described bore any appendages on their stems. *Rhynia Gwynne-Vaughani*, though smaller than *R. major*, shows a feature of morphological advance towards something in the nature of appendages. The upright stems bifurcate as before, bearing distal sporangia similar to, but smaller than, those of *R. major*.

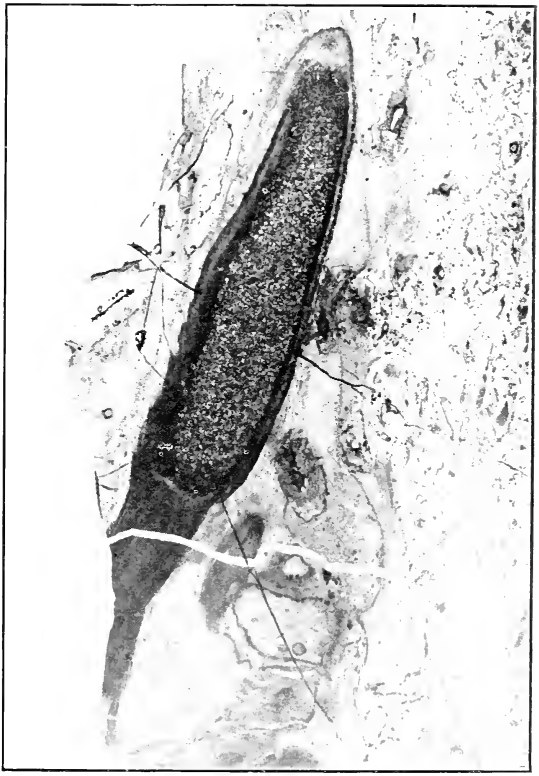

FIG. 4.—SPORANGIUM OF *Rhynia major*, FILLED WITH SPORES ($\times 5\frac{1}{4}$).

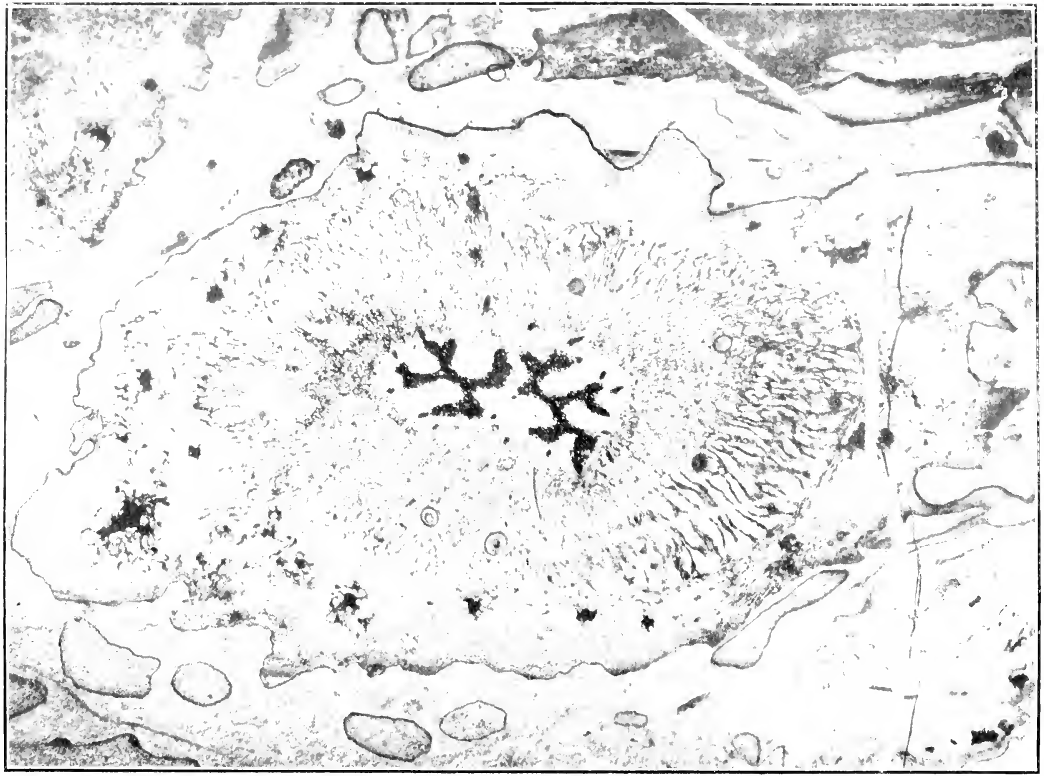

Fig. 5. Large Stem of *Asteroxylon*, cut transversely just below a Dichotomy, and showing Leaves attached externally (× about 10).

But near to their base there are "hemispherical projections," apparently of superficial origin. Some of these gave origin to tufts of hair, but others produced adventitious branches, which having narrow bases were easily detached, and served as means of vegetative propagation. Though these organs are not easily ranked with those of living plants, they are something in advance of what is seen in *Hornea* and *R. major*. The sporangia are relatively small, and there is no clear evidence of their dehiscence.

The largest, as it is also the most complex, of these plants is *Asteroxylon Mackiei*. Its base consisted of branched rhizomes, which burrowed after the manner of Stigmarian rootlets, and each was traversed by a vascular strand with undifferentiated xylem: but curiously enough rhizoids are absent. These rhizomes passed over into upright aerial stems, which attained a diameter of as much as a centimetre, and had a complex structure (Fig. 5). They forked, and bore externally small and simple leaves. The stele had a stellate xylem very like some Lycopods. From its rays issued strands passing to the bases of the leaves, but not entering them. As in *Lycopodium*, more than one vertical series of leaf-traces may issue from each ray of the stellate xylem, a fact that confirms the Lycopod comparison. Longitudinal sections show the relations of epidermis, cortex, phlöem and xylem, and the way in which the inner cortex of stem and rhizome often contain fungal hyphæ. It is possible that in the rhizome these may have been concerned in mycorhizic nutrition. Higher powers demonstrate the tracheides as irregularly, or spirally barred, but not scalariform. An endodermis has been seen delimiting the cylindrical stele, and mesarch protoxylem is found in the xylem-rays. The leaves are parenchymatous, the vascular strands stopping short at their bases. The epidermis has been found to bear very perfect stomata. The essential points of structure of the plant are thus fully known.

In certain blocks sporangia have been found attached to profusely dichotomising stalks of simpler structure than the main stems of *Asteroxylon*, and not definitely attached to them. They are associated, however, with stems of *Asteroxylon*, while those of *Hornea* and *Rhynia*, from which they are structurally distinct, are absent from the blocks. The association makes it probable that these peculiarly forked branches and sporangia really belong to *Asteroxylon*. The sporangia are relatively small and pear-shaped, and they had a distal dehiscence. The whole plant of *Asteroxylon* was thus more advanced in various respects than any of the other three plants of the chert.

Comparison of these four fossil species from Rhynie with other fossils already known from the Lower Devonian Period shows that a very homogeneous flora existed at that time, consisting chiefly of leafless and rootless land-living plants. These and other characters, such as their large, distal, sometimes solitary, and often forked

sporangia, stamp these plants as exceptionally primitive. Among living plants the nearest of kin to them are clearly the Psilotaceæ, a family which has long presented a problem in morphology and classification. It comprises two living genera, *Psilotum* and *Tmesipteris*. Both genera are rootless. Their imperfect morphological differentiation is shown by the fact that botanists are not yet agreed whether their lateral appendages are to be held as truly foliar or not. *Psilotum* is native throughout the tropics, and is represented by two well-marked species. The commonest, *P. triquetrum*, has upright and shrubby aerial shoots, with radial construction and frequent bifurcations. These spring from leafless underground rhizomes, profusely bifurcated. They are covered with rhizoids, and contain a mycorhizic fungus. On the lower part of the aerial shoots simple spine-like leaves are borne, but towards the distal ends these are replaced by forked spurs, between the prongs of which a synangium, usually with three loculi, is seated. The aerial shoot is traversed by a vascular strand consisting of xylem in the form of a hollow, many-rayed star with sclerotic core, and branch-strands run out to the appendages. The whole is covered by epidermis with stomata, and the cortex provides the photosynthetic tissue. *Tmesipteris* is represented by only one species, limited to Australasia. It grows usually among the massed roots that cover the stems of tree-ferns, but sometimes upon the ground. Its general form is like that of *Psilotum*, but the underground rhizomes are longer and the appendages larger, while only two loculi are usually present in each synangium. Clearly the form and vascular structure of these plants is generally like that of the Rhynie flora.

Till quite lately the Psilotaceæ remained the only living Pteridophytes of which the life-cycle was still incompletely known. In all the other groups the regular alternation of two generations had been demonstrated; the one is the prothallus, which is sexual, and the other is the established plant, which is non-sexual. In the Psilotaceæ also the plant as above decribed is the non-sexual generation, but hitherto the form or even the existence of the sexual generation remained problematical. Since 1914 the prothalli of both genera of the Psilotaceæ have been discovered, and their structure demonstrated by Darnell-Smith, Lawson, and Holloway. And so the very last of these life-histories has now been completed. It turns out that the prothallus of the Psilotaceæ is similar in its general characters to those of other archaic Peridophytes, being colourless, and living in humus by means of fungal nourishment. In fact, these plants conform in their life-cycle to what is seen in the Lycopods and in the primitive Ferns. Analogy with the living Psilotaceæ makes it highly probable that the Lower Devonian plants also showed alternation. Though this has not been demonstrated for them, their preservation is so perfect that even the delicate prothallus may yet be revealed as the reward of further search.

The interest of the recent work on the modern Psilotaceæ centres not so much in the details of the prothallus as in their embryology. It has been shown by Holloway that the embryo of *Tmesipteris* is rootless from the first. This suggests that the rootlessness is primitive, and not the result of reduction. Since the Lower Devonian plants were rootless also, it seems probable that this state was characteristic of such early plants of the land. Further, the existence of *Sporogonites*, and the very moss-like structure of its sporangium, together with its similarity to the sporangia of *Rhynia* and *Hornea*, seem to link up the latter naturally with the Bryophytes, which are also rootless. In fact, we see before us a flora of rootless plants, which raises afresh the question of the first establishment of the neutral generation as an independent, soil-growing organism. It originates in every case within the tissue of the sexual plant, and is at first dependent upon it. This condition is seen in the embryo of *Tmesipteris*, with details not unlike those of the Anthoceroteæ. How, then, did it first establish itself independently upon the soil?

This question was first raised long ago by Dr. Treub, the brilliant Director of the Botanic Gardens at Buitenzorg. He suggested that in the evolution of land-living plants a rootless phase would naturally precede the full establishment of the sporophyte in the soil. He saw this reflected in the embryonic state of certain Lycopods, where a parenchymatous tuber precedes the establishment of the rooted plant. It is attached to the soil by rhizoids, and contains a mycorhizic fungus. This tuber Treub styled the "protocorm." He regarded it as a general precursor of the established leafy plant in descent. During the war new examples of this protocorm-stage have been described by Holloway, which show the condition in its most pronounced form. In *Lycopodium laterale* it constitutes the whole plant-body for the first season. It bears numerous protophylls, and may even branch, and reproduce itself vegetatively. It is only later that the leafy shoot and lastly the root are formed. The fact that *Hornea* shows a similar tuberous swelling at the base of the rootless plant, and retains it even in the adult state, brings the added interest that a permanent protocorm figures in the earliest known land flora. Its antiquity is thus undoubted. But the Lower Devonian plants do not all show it in a distended form. The tuberous swelling is not conspicuous in *Rhynia* or in *Asteroxylon*, and it is significant that in the living *Tmesipteris* the rhizome is cylindrical. These facts indicate that the distended protocorm is neither an obligatory nor a constant feature.

It will not be necessary to do more than refer briefly to the controversy whether the appendages of the Psilotaceæ are truly leaves or branches. The fact suffices that the question has been in debate, and that similar questions arise in relation to the fossils of the Lower Devonian. In them it is impossible to assign the name leaf to any definite part in the full sense in which it is used in the higher vascular plants. The difficulties of their morphological

analysis and their rootlessness are in themselves evidence of the primitive state of these fossils. We are, in fact, in the presence of what evolutionists call *Synthetic Types*—that is, such as link together groups which have diverged. The Lower Devonian plants and the Psilotaceæ show us just those forms which might have been anticipated as a consequence of comparative study, and some of their characters were actually forecast by Dr. Treub.

Though it may be difficult to place the parts of these synthetic types in the categories of stem, leaf, and root, as those terms are applied to more advanced forms, still they will serve to illuminate the probable origin of these parts. The rhizomes of *Asteroxylon* suggest an origin of roots from branched, leafless rhizomes. Its "leaves" suggest a relation with the leaves of Lycopods; but its most significant feature is the branch-system ascribed to *Asteroxylon*, bearing the distal sporangia, which is so like that already described for the enigmatical Carboniferous fossil *Stauropteris*. This comparison has already been pointed out by Kidston and Lang. On the other hand, approaching the question from the side of the living ferns, I indicated in 1917 that "the distal and marginal position of a sorus, often monangial, is prevalent among primitive ferns, and that more complex sori are referable in origin to it." Comparison of the distal sporangia of the Psilophytales with those of *Stauropteris*, *Botryopteris*, the Ophioglossaceæ, *Osmunda*, and the Schizæaceæ, gives a sequence which sketches in broad lines, though not monophyletically, a probable origin of marginal sporangia for the ferns. It is accompanied by reduction of size and spore-number in the later and derivative types, which is continued on to the most advanced of living ferns. A reduction of the distal branchlets to a single plane, and webbing of them laterally together, would give a type of sporophyll and fructification known in certain primitive ferns. But if this were the real course of their evolution, the sporophyll so constructed would be a different thing from the "leaves" seen in *Asteroxylon*. This was the vision of the prophetic Lignier, who has not lived to see his ideas tested by these new discoveries. But such comparisons still leave in doubt the origin of the axis in fern-like types. It is not clear yet how near the truth for them my suggestion of 1884 may be: that "the stem and leaf would have originated simultaneously by differentiation of a uniform branch-system into members of two categories." Nevertheless, the important new fact, which now gives reality to this theory, is that a uniform branch-system has been shown to have existed in these early vascular plants. A sympodial development of it, after the manner shown in the leaves of living ferns, would provide at least one type of foliar appendage, which would bear a relation to the axis similar to that of the pinnæ to the phyllopodium or rachis of the leaf.*

* Phil. Trans. 1884, p. 565.

On the other hand, comparison of the Bryophytes will leave little doubt that the sporangium of the Psilophytales and the sporogonium are kindred structures. If this be so, then we shall see linked together by comparison with these new fossils, not only the sporogonia of Bryophytes and the sporangia of ferns, but even the pollen-sacs and ovules of flowering plants. Long ago it was remarked that the widest gap in the sequence of plants was that between the Bryophytes and the Pteridophytes. It is within this gap that the newly discovered fossils take their natural place, acting as synthetic links, and drawing together more closely the whole sequence of land-living, sporangium-bearing plants. We still await with interest the considered comparisons of the authors of these notable memoirs, though they have already pointed out several fertile lines. But those who have been deeply engaged in comparative morphology may be excused for stating how these new facts strike them. Clearly the morphology of land-living plants is again in the melting-pot. It will emerge strengthened by new and positive facts, and refined by comparisons which can now be based upon solid data, and less than before on mere surmise.

The new facts are thus seen to link the Bryophytes and the Pteridophytes more closely together than ever before. It may be that these two great phyla of land-living plants have themselves diverged from some common source still unknown. But that source is reflected more nearly in the Lower Devonian plants than in any other known forms. If that be so, whence may these still more primitive plants have sprung? The view has always been entertained that the Algæ preceded land-living plants. For long the fresh-water green Algæ were believed to have provided the source. Latterly from the Continent, but notably also here at home, at the instance of Lang and of Church, the belief has swung round towards marine forms. Highly specialized Algæ flourish on every rocky shore. Some of these show alternation. All are rootless. Some have a differentiation of their branch-system which prefigures the relation of leaf and axis. Not a few of the Red Seaweeds have spore-tetrads borne internally, and located in the ends of specialized branches called *stichidia*. These are not altogether unlike sporogonia, or the large sporangia of the Lower Devonian plants. We may well regard it as improbable that any direct transition of such specialized types to a land-habit took place, though this has been hinted at more than once. But at least corresponding features of external differentiation and of spore-production are present in both. Homoplasy may be the real explanation of the likeness, but still the similarity exists.

From what has been said it is clear that during the years of war plant morphology has entered upon a new phase. The problems of origin of root and axis and leaf and sporangium have been propounded afresh in terms of the new discoveries. The day is past of that vague surmise on these points which has bulked so largely in

the discussions of recent decades. It was the paucity of facts that kept opinion in suspense, hovering between rival arguments rather than settling on assured data. Looking back upon the history of that branch of Botanical Science which is called Comparative Morphology, there is only one period that can rival the years from 1913 to 1920 in point of positive advance. It is the period which led up to the great generalisations of Hofmeister sixty years ago. In the glories of that work Britain had no direct share, though it was carried out at the very time when Lyell, Darwin, Wallace, Hooker, and Huxley were laying the theoretical foundation which gave their real significance to the discoveries then made by Hofmeister. In the words of Sachs: "When Darwin's theory was given to the world . . . the theory of Descent had only to accept what genetic morphology had already brought to view." Science it is true is cosmopolitan, and should always be held as such. But still we in Britain may feel a legitimate satisfaction that in these recent discoveries, which have transformed the problems of Morphology, the material, the observations, and the arguments based upon them are mainly of British origin. The channel of publication of the results, so largely derived by Scottish workers from Scottish material, has naturally been the Transactions of the Royal Society of Edinburgh.

[*Note.*—The Figures 1–5 have been prepared from the original photographs of Dr. Kidston, F.R.S. The use of these illustrations is gratefully acknowledged. They are taken from the three Memoirs "On Old Red Sandstone Plants showing Structure, from the Rhynie Chert Bed, Aberdeenshire," by R. Kidston, LL.D. F.R.S., and W. H. Lang, D.Sc. F.R.S., published in the Transactions of the Royal Society of Edinburgh, Vol. XLI. Part 3; XLII. Part 3.]

[F. O. B.]

ANNUAL MEETING,

Thursday, May 1, 1920.

Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S., Treasurer, in the Chair.

The Annual Report of the Committee of Visitors for the year 1919, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Sixty-four new Members were elected in 1919.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1919.

The Books and Pamphlets presented in 1919 amounted to 193 volumes, making with 399 volumes (including Periodicals bound) purchased by the Managers, a total of 592 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

President—The Duke of Northumberland, M.V.O. C.B.E.

Treasurer—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

Secretary—Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S.

Managers.

Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. D.L. J.P. LL.D.
John Y. Buchanan, F.R.S.
W. A. Burdett Coutts, M.P.
The Right Hon. C. Scott Dickson, P.C. K.C. LL.D.
Sir James J. Dobbie, LL.D. D.Sc. F.R.S., Pres. C.S.
J. Dundas Grant, M.D. F.R.C.S.
Donald W. C. Hood, C.V.O. M.D. F.R.C.P.
The Right Hon. Earl Iveagh, K.P. G.C.V.O. LL.D. F.R.S.
H. R. Kempe, M.Inst.C.E.
Sir Ernest Moon, K.C.B. K.C. LL.B.
The Hon. Sir Charles Parsons, K.C.B. D.Sc. LL.D. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Sir Ernest Rutherford, LL.D. D.Sc. F.R.S.
Sir Henry Wood.

VISITORS.

Sir Hugh Bell, Bart., C.B. D.C.L. LL.D. D.L.
Sir William H. Bennett, K.C.V.O. F.R.C.S.
W. R. Bousfield, K.C. F.R.S.
John G. Bristow, M.A.
Frank Clowes, D.Sc. F.C.S.
Montague Ellis.
W. E. Lawson Johnston.
John R. Leeson, J.P. M.D. C.M.
Thomas Bell Lightfoot, M.Inst.C.E.
F. K. McClean, F.R.A.S.
William Smith Norman.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.
Thomas H. Sowerby, B.A.
Sir Almroth Wright, K.C.V.O. K.B.E. C.B. M.D. Sc.D. F.R.S.

GENERAL MONTHLY MEETING,

Monday, May 3, 1920.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

The Chairman announced that His Grace the President had nominated the following Gentlemen as Vice-Presidents for the ensuing year :—

J. H. Balfour Browne, K.C. D.L. J.P. LL.D.
W. A. B. Burdett-Coutts, M.P.
Donald W. C. Hood, C.V.O. M.D.
The Right Hon. Earl Iveagh, K.P. G.C.V.O. LL.D. F.R.S.
The Hon. Sir Charles Parsons, K.C.B. J.P. D.Sc. LL.D. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S. (Treasurer)
Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S. (Secretary)

Sir John Cadman, K.C.M.G. D.Sc. M.Inst.C.E.
Miss Nina de Lara,
Mrs. Duncan Mackinnon,
The Hon. Lady Parsons,
Lieut.-Col. W. T. Raikes, D.S.O. M.C.
Marcus R. A. Samuel,
Robert Edward Thomas, M.A.

were elected Members of the Royal Institution.

The Chairman announced the decease of Dr. R. Messel, on April 18, and the following Resolution, passed by the Managers at their Meeting held this day, was unanimously read and adopted :—

RESOLVED, That the Managers desire to record their sense of the loss sustained by the Institution and the world of Industrial Chemistry by the death of Rudolph Messel, F.R.S., President and Foreign Secretary of the Society of Chemical Industry, 1913–1917; Vice-President of the Chemical Society; Member of the Board of Studies, University of London; Member of Governing Body, Imperial College of Science and Technology; and formerly a Manager and Visitor of the Royal Institution.

Dr. Messel was elected a Member of the Royal Institution in 1890, and was a constant attendant at the Lectures for a period of thirty years. He was a man of high scientific attainments, and took great interest in the internal equipment of the Institution, presenting a bronze bust of Davy in 1900, and was a generous contributor to the Research Fund. The name of Messel will always be identified with his brilliant invention of the Industrial Synthetic Production of Sulphuric Anhydride by what is known as the Contact Process, thereby creating in this country a new and most important industry of a vast importance in production of colouring matter from coal tar, and various other industries. His success stimulated scientific enquiry, which has extended the study of contact reactions to the production of bodies of organic and inorganic origin, which is reflected in the newest and most important synthetical industries developed in recent years.

The Managers desire, on behalf of the Members, to express their deep sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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  Agricultural Research Institute, Pusa: Bulletin, Nos. 91, 92, 94. 8vo. 1920.
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Zoological Society—Report for 1919. 8vo. 1920.

WEEKLY EVENING MEETING,

Friday, May 7, 1920.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

THE RIGHT HON. LORD RAYLEIGH, Sc.D. F.R.S. M.R.I.,
Professor of Physics, Imperial College of Science.

The Blue Sky and the Optical Properties of Air.

Scattering by Small Particles. Polarisation.

THE subject chosen for this evening is one which specially interested my father throughout his career. I shall try to put before you some of his conclusions, and then pass on to more recent developments, in which I have myself had a share.

Let us begin with one of his experiments which illustrates the accepted theory of the blue sky. We have here a glass tank containing a dilute solution of sodium thiosulphate. A condensed beam from the electric arc traverses it and then falls on a white screen, where it shows the usual white colour. I now add a small quantity of acid, which decomposes the solution with slow precipitation of very finely divided particles of sulphur. As soon as this precipitation begins you see that light is scattered—that is to say, that it is diverted to every side out of the original direction of propagation. Moreover, you will observe that the scattered light is blue. The transmitted beam is robbed of its bluer constituents, and tends to become yellower, as you may see on the screen.

The light scattered laterally is to be compared to the blue sky; the yellow transmitted light to the direct light of the setting sun, when it has traversed a great thickness of air.

As the precipitation goes on, the transmitted light becomes orange, and even red. But the particles of sulphur eventually get bigger, and then give a less pure blue in the lateral direction. We shall have more than enough to occupy us if we confine our attention to the earlier stages, when the particles are small compared with the waves of light.

A very important property of the scattered light is its polarisation. The vibrations of the scattered light as you have seen it, viewed laterally in the horizontal plane, are almost wholly up and down. No light is emitted which vibrates in the horizontal plane. It is easy for individual observers to verify this with a Nicol's prism held

to the eye, but this direct method unfortunately does not lend itself to public demonstration.

We may, however, use polarised light to begin with, and you can then observe that if the polarising Nicol is set so as to transmit up and down vibrations, these are abundantly scattered towards you by the small particles. As I turn the polarising Nicol through a right angle, you will see that the light scattered towards you is extinguished.

The polarisation of light scattered by the sulphur particles is one of the most conclusive reasons for considering it to be an analogue of the blue light of the sky, for the latter shows a polarisation of exactly the same kind when examined at right angles to the sun.

A cloud of small particles of any kind is capable of producing these effects, the essential condition being that the individual particles should be of small dimensions compared with the wave-length of light, so that at a given moment the vibration at a given particle may be regarded as having a definite phase. In this case it was shown by my father that the shorter (blue) waves are of necessity more scattered than the longer ones (red); thus the scattered light is bluer than the original. This conclusion can be justified in detail whether we adopt the elastic solid theory, or the electro-magnetic theory of the nature of light, but it is also deducible from the general theory of dimensions, without entering upon any details of the nature of light beyond its characterisation by the wave-length.

An alternative theory which still sometimes shows its head, attributes the colour of the sky to a blueness of the air, regarded as an absorptive medium. Such blueness is referred to the presence of ozone, and appeal is made to the undoubted fact that a sufficiently thick layer of ozone shows a blue colour by absorption. This theory gives no account of why the sky light is polarised, or indeed of why there is any light in the clear sky at all. Further, its fundamental postulate that the air is blue by transmission is contrary to observation. The setting sun is seen through a greater thickness of air than the midday sun. According to the theory under discussion, the setting sun ought to be the bluer of the two, which everyone knows it is not. No doubt the presence of ozone tends to make the air blue by transmission. But this effect is more than compensated by the lateral leakage (scattering) of blue light from the beam, which makes the transmitted light yellow.

Dusty Air and Pure Air.

If it be conceded that the blue sky is due to scattering by small particles, we are confronted with the question of what nature are these particles? At the time of my father's early investigations (1871) this was left open, though they were regarded as extraneous to the air itself. In 1899 he returned to the subject, and considered the matter from the point of view of what was lost by the original beam

by lateral leakage (scattering), which simulates the effect of absorption. He then found that the air itself, regarded as an assemblage of small particles (molecules of oxygen and nitrogen), would have an apparent absorbing power not much less than that actually deduced by observations of the sun at different altitudes. The inference was that the air itself was capable of accounting for much, if not all, of the scattering which is observed in the blue sky; in fact that the molecules of air are the small particles in question.

When a beam of sunlight enters a room through a small aperture in the shutter, its course is readily traced by the brightly illuminated motes in the air. Prof. Tyndall, working in this Institution, devoted much attention to the nature of these motes, and the methods by which they may be got rid of. His results may be consulted in the fascinating essay on "Floating Matter." One way of getting rid of the motes is to filter the air through cotton wool. We have here one of Tyndall's own experimental tubes. The electric beam passes axially along it, and is concentrated to a focus about the middle of the length. Its track is conspicuous. If now we displace the air originally in the tube by filtered air, you see that the cone of light fades into invisibility.

Another of Tyndall's experiments was merely to place a spirit-lamp or Bunsen burner under the beam. Since most of the dust particles are combustible, the gases rising from the flame are free from them. As you now see, dark rifts appear in the beam where the uprising stream of dust-free gases traverses it.

Tyndall, on the strength of these experiments, stated without qualification that dust-free air does not scatter light, but my father's views and theory lead clearly to the conclusion that it does. But when I asked him what he thought about the feasibility of detecting it by a laboratory experiment, he was not very sanguine of success. It seemed worth while, however, to make the attempt, and I came to the conclusion that the difficulty was not so much in the faintness of the effect to be looked for as in the avoidance of stray light which came into competition with it. The essential thing is to get a perfectly black background against which the beam (viewed transversely) can be observed. We cannot get this with a vessel like Tyndall's tube, just used. It is necessary to have what may be called a black cave, and to view the beam as it crosses in front of the mouth of the cave, the latter forming the background. If the cave is deep enough, there is no limit to the blackness attainable. The great sensitiveness of the well-rested eye, or the photographic plate, can then be brought to bear, and the track of the beam can be well seen, however carefully the dust is removed.

Some persons have been inclined to question whether the dust is removed completely in these experiments. As a matter of fact this is not where the difficulty lies at all. Dust so fine as to be very difficult of filtration is an arm-chair conception, not encountered in

practical experimenting. An enormous multiplication of the length and tightness of the cotton-wool filter makes no difference at all ; a filter of modest dimensions doing all there is to do.

The dust particles which are originally present in the air, near the ground or in a room, are large, being in some cases individually visible to the naked eye : thus they do not fulfil the condition for scattering a preponderance of blue light. The molecules of air are of course amply small enough, and the band of light seen stretching across the mouth of the dark cave is, to my eyes at least, of a full blue colour. In exhibiting the effect to individual friends (and unfortunately it is not bright enough to be shown to an audience), I have been surprised and somewhat disconcerted to find that they do not all see it blue as I do, but some, for example, describe it as lavender. This is undoubtedly due to a peculiarity of colour-vision where faint lights are concerned. The ultimate test is the spectroscope. Photographs of the scattered light taken with this instrument clearly show that the maximum of intensity is shifted towards the blue, as compared with the original exciting light.

Polarisation of Light Scattered by Pure Air.

A very important point to examine in connection with the scattered light is its state of polarisation. Visual examination with a Nicol's prism soon showed that the polarisation was very nearly complete. For closer examination I had recourse to photography. It may perhaps be thought an easier and more effective plan to look at a phenomenon than to photograph it, and no doubt it is so in many cases ; not however where the light is very faint, but admits of long exposure. It has long been recognised that photographs of the nebulæ will show much more than can be detected visually by the keenest and most discriminating eye. In this work on the scattering of light, I have found it positively less trouble to take a photograph than to make a visual observation, even when the latter was feasible. The time required to rest the eye in darkness and the effort of attention required in observing a faint effect cost the experimenter more effort than the exposure and development of a plate.

When the scattered beam in pure air is photographed, with a double image prism of Iceland spar mounted over the photographic lens, it is found that the polarisation is nearly complete, but not absolutely so. However carefully the instrumental adjustments are made, and however carefully the air is filtered, I have found that there is a slight residual polarisation indicating vibrations parallel to the direction of the original beam. The intensity of this residual polarisation, in what may be called for convenience the wrong direction, is about 4 per cent. of the whole. Now, as the theory shows, there are two causes to which failure of complete polarisation may be attributed. One, which we may dismiss in this case, is that the

particles are not small enough. Another is that they are not spherical—that is to say, that it is not a matter of indifference which way they are presented to the primary beam. The latter alternative may be illustrated by considering an extreme case—namely, what we may call a needle-like molecule, capable of vibrating only in one direction fixed within it. Evidently such a molecule when obliquely situated will have a component vibration parallel to the direction of the incident light.

From the experimental fact that there is such a component, we may infer that the molecules of air are not in the optical sense spherical. Experiments on various gases have shown a characteristic departure from complete polarisation, different for each gas. Much effort has been spent on determining the exact amount for each, and it is hoped that the numbers obtained will form valuable material in the future for investigating the structure of atoms and molecules.

Polarisation of the Night Sky.

We have seen that the polarisation of the daylight sky is one of the most conclusive proofs that its light is due to scattering by small particles. What of the sky at night? Some of you will perhaps be inclined to reply that the sky at night is dark, and that the question whether its light is polarised does not arise. It is, however, by no means the case that the sky on a clear night is absolutely dark, as anyone may readily prove by holding his hand with outstretched fingers against the sky. The fingers will appear dark against the sky as a luminous background.

The light is no doubt very faint, but I thought it would be practicable to test whether it was appreciably polarised or not. For this purpose what is called a Savart polariscope was used. Time will not allow us to consider the rather complex theory of this apparatus; it must suffice to say that if the light which falls upon it contains even a small part which is polarised, alternately bright and dark bands are produced, which further show colour due to the composite nature of white light. These bands are clearest when the incident light is completely polarised, as you now see them projected on the screen. But they can still be seen when the polarisation is but slight. I will illustrate this by removing the polarising Nicol which I have been using, and substituting a single glass plate, through which the incident light passes. If I incline this plate, so as to polarise a small fraction of the light, you see the bands, faint but sufficiently distinct. In examining the light of the night sky, a photographic plate is substituted for the paper screen I have been using to-night, and the apparatus is designed for the utmost economy of light. With two hours' exposure a definite image of the sky was obtained, with the stars superposed upon it. The Savart bands could be seen, but they were very faint compared with what

would have been observed with an equally good image of the daylight sky. The part of the sky examined was near the pole, and therefore nearly at right angles to the sun. If, as seemed possible, the night sky derived its light from an attenuated atmosphere so high as to be outside the earth's shadow, we should expect it to show the same polarisation as the day sky. Since it does not do so, we must attribute the light at night to some different origin.

I was fortunate in being able to interest Prof. Hale in this matter while he was on a visit to England, and as a result Mr. Babcock repeated the observations in a modified form at the Mount Wilson Observatory in California. The traces of polarisation which he obtained in that clear atmosphere were even less than what I got in England.

Ozone, and the Limit of the Solar Spectrum.

Although, as we have seen, the idea that the blue colour of the sky is due to any action of ozone cannot be admitted, yet there are points of great optical interest connected with the presence of this gas in the atmosphere. We may now turn to the consideration of some of these.

It is of course well known that when the solar spectrum is formed by a prism of quartz, or by a grating, the spectrum can be observed to extend beyond its visible limit in the violet into the region called ultra-violet. When, however, we examine the spectrum of an electric arc (and for this purpose an iron arc is particularly suitable), the extension is observed to be very much greater than in the solar spectrum. This is not because the sun does not emit any rays of the kind in question, but because the earth's atmosphere will not allow them to pass through so as to reach us at the earth's surface. There are many reasons for feeling sure that this is the true explanation, but one of the simplest will here suffice. When the sun is near the horizon, so that the rays pass obliquely through the earth's atmosphere, and consequently have to traverse a thicker absorbing layer, the extent of the ultra-violet spectrum is found to be even less than when the sun is high and less air is traversed by the rays. This sufficiently proves the point.

It has long been suspected that ozone in the atmosphere is the effective cause of this absorption of the ultra-violet rays. The most important constituents of air, oxygen and nitrogen, do not appreciably absorb at the point where the solar spectrum ends, nor do the constituents of secondary importance, carbonic acid, water-vapour and argon. We must therefore look to some rare constituent of air which is very opaque to this region of the spectrum. Ozone possesses this opacity, as I shall now show you. So far as I know it has not been attempted to show this before to an audience, but I think you will be able to see it without difficulty. As a source of light an iron arc is used, and the lenses and prism used in forming the

spectrum are of quartz. I allow the spectrum to fall on a piece of paper, and you see the usual succession of colours, red, yellow, green, blue and violet, forming a comparatively narrow rainbow-like band. Beyond the violet all appears dark, the eye being insensitive to the ultra-violet rays. If now I substitute for the paper a screen of barium platinocyanide * (of the kind used in X-ray work), we see an immense extension of the spectrum beyond the violet. The screen has the property of transforming the ultra-violet rays, which the eye cannot detect, into green rays which are readily visible. Thus beyond the violet region we see green, which is, of course, in no way to be confused with the original green which was present in the source, and appears in its normal position in the spectrum, on the other side of the blue-violet. I interpose a thin sheet of ordinary glass, and the greater part of this extension of the spectrum which we get on the fluorescent screen disappears. What I want specially to show you, however, is that a thin layer of ozone, much too thin to have any perceptible colour, will have the same effect. There is a glass tube, about 6 inches long and $\frac{3}{4}$-inch diameter, situated between the quartz lantern condenser and the slit, when the beam is parallel, and the walls of the tube are projected as two thin transverse lines on the slit, dividing the spectrum into thin horizontal strips, one over the other. The light constituting the middle strip has traversed the tube, but the light constituting the upper and lower strip has traversed the open air above and below the tube. A stream of oxygen passes through a Siemens' ozone generator and enters the middle of the observation tube, streaming out at the two ends. While the ozone generator is not excited, the middle strip of the spectrum is similar to the comparison strips above and below. If the induction coil is turned on so that ozone passes into the tube, you see that in a few seconds the greater part of the ultra-violet spectrum fades out from the middle strip, which contrasts sharply with the upper and lower ones. When the coil is turned off, the ozone is rapidly blown out by unozonised oxygen, and the original state of things restored.

It must be remembered that the ozone used in this experiment is extremely dilute, probably only a fraction of 1 per cent. of the oxygen in the tube. Yet it interposes an impassable obstacle to the ultra-violet rays, at least to those of shorter wave-length than about 2900 Ångstroms. It cuts off the iron spectrum at about the same point where the solar spectrum ends. Speaking roughly and generally, it may be said that glass is somewhat more opaque than ozone, i.e. that with diminishing wave-length the limit of transmission is reached somewhat sooner. To make a statement of this kind quite definite the thickness must of course be specified.

Sir William Huggins devoted a great deal of attention to the

* Kindly lent by Messrs. Watson.

spectra of the sun and stars in the extreme ultra-violet region, using for the purpose a reflecting telescope, and prisms and lenses made of quartz or Iceland spar. In this way the absorption of a glass objective was avoided. He noticed in 1890 that the spectrum of Sirius showed a number of bands near the extreme limit of atmospheric transmission, the bands tailing off into complete absorption.

These bands were observed and discussed by other authors, but no definite conclusion was reached as to their origin until 1917, when the matter was taken up by my colleague, Prof. Fowler, and myself. Our interest was stimulated by an excellent photograph of the bands, taken at Edinburgh Observatory under Prof. Sampson's direction, which I show on the screen. We found that the same bands were present in the solar spectrum. It may seem strange that this had not been observed long ago, considering how closely the solar spectrum has been scrutinised for more than a generation. As a matter of fact this is one of the cases where a powerful instrument is a positive disadvantage. The bands are diffuse, and under high dispersion they are unrecognisable. In any case they are less conspicuous than in the spectrum of Sirius, because in the sun numerous metallic lines are superposed upon them and distract the eye.

Now the position and general aspect of these bands suggested that they were connected with the absorption which terminates the spectrum. This led us to suspect that they were due to ozone, and the suspicion was readily confirmed by experiment. Burning magnesium ribbon gives a convenient source of continuous spectrum in the ultra-violet region. Interposing a long tube containing ozone between the burning magnesium and the slit, a series of bands was photographed which exactly corresponded to those photographed in the solar spectrum with the same instrument, as you will see in the slide shown.

Absence of Ozone Near the Ground.

We are driven then to the conclusion that the absence of short waves from the spectra of the sun and stars is due to absorption by terrestrial ozone. But it was not thought desirable to let the matter rest there. It is true that many attempts had been made to determine the (no doubt very small) quantity of ozone in air by chemical means, but with very conflicting results, because other constituents of air, such as oxides of nitrogen, are liable to produce reactions not unlike those of ozone. It seemed more satisfactory to test the absorbing power of air near the ground for ultra-violet rays, to which ozone is so opaque. I used for this purpose a mercury vapour lamp in a quartz vessel, which is a powerful source of ultra-violet rays, and observed its spectrum four miles away, so that the mass of air intervening was as great as that between the midday summer sun and the top of the Peak of Teneriffe, from which observations of the extent of the solar spectrum have been made. The result was to

show that the mercury lamp spectrum was by no means stopped when the solar spectrum stops, but that it extended to the region where ozone is most opaque. There is a strong mercury line (wave-length 2536) at about this point which was distinctly photographed. Its intensity was of course a good deal reduced relative to the visible spectrum by atmospheric scattering. But there was no evidence whatever of ozone absorption.

What conclusion can we draw? Evidently that the absorbent layer of ozone in the air is high up, and that there is little or none near the ground. It may seem at first sight that this thin and inaccessible layer of ozone, of which we have learned by a chain of reasoning not less conclusive than direct observation, is a matter of little importance to man and his welfare. There could be no greater mistake. It acts as a screen to protect us from the ultra-violet rays of the sun, which without such a protection would probably be fatal to our eyesight, at least if one may judge from the painful results of even a short exposure to such rays, which those who have experienced it are not likely to forget.

[R.]

WEEKLY EVENING MEETING,

Friday, May 14, 1920.

Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Karl Pearson, F.R.S.

Sidelights on the Evolution of Man.

[For Report of Lecture see University of London Galton Laboratory for Natural Eugenics Lectures, Series XIII. (1921)].

WEEKLY EVENING MEETING,

Friday, May 21, 1920.

HIS GRACE THE DUKE OF NORTHUMBERLAND, M.V.O., President, in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S. M.R.I.,
University Professor of Electrical Engineering
in the University of London.

The Thermionic Valve in Wireless Telegraphy and Telephony.

THE Thermionic Valve is an invention which has vastly increased the powers and range of wireless telegraphy.

It has provided simple and efficient means for conducting wireless telephony over great distances, even across the Atlantic Ocean. It has given to telephonic engineers a very perfect form of relay or repeater by which it is possible to transmit speech through copper wires of given section to greater distances than without it, or conversely to reduce considerably the amount of copper required for good speech over the same distance. It has placed a new implement in the hands of the scientific investigator, aiding him in various branches of research, and lastly it presents itself as an object of scientific enquiry by no means exhausted of all that it has to teach.

Like many other inventions, the telephone for instance, it is simple in its essential construction. It consists of a little electric lamp comprising a glass bulb, very highly exhausted of its air, containing a filament of carbon, or better tungsten, which can be rendered incandescent by an electric current. Within the bulb and around the filament are fixed certain metal plates or cylinders, and it may be spirals of wire or metal networks called the grid.

To explain its origin in its simplest form I shall have to take you back in thought to the days when the physical effects taking place in incandescent electric lamps were first beginning to be carefully considered.

More than thirty years ago I had the honour of giving a Friday Evening Discourse in this theatre on some "Problems in the Physics of an Electric Lamp," and for the sake of those now present to whom the subject is new, I shall repeat one or two of the experiments then shown as a starting point for further explanations.*

* See The Proceedings of the Royal Institution of Great Britain, vol. xiii, p. 34, Feb, 14, 1890, "Problems in the Physics of an Electric Lamp."

In 1883 Mr. Edison for some purpose placed in the glass bulb of one of his carbon filament lamps a metal plate which was carried on a platinum wire sealed through the glass. When the filament was rendered incandescent by a current from a battery, he found that if the plate was connected by a wire, external to the lamp, with the positive terminal of the filament, a small electric current flowed through it, but if connected to the negative terminal no current, or at most a very feeble current, flowed (Fig. 1).

This new and interesting effect became known as the "Edison effect" in glow lamps, but Mr. Edison gave no explanation of it and made no practical application of it in telegraphy, or for any other important purpose.

Edison supplied some lamps with plates in the bulb to the late

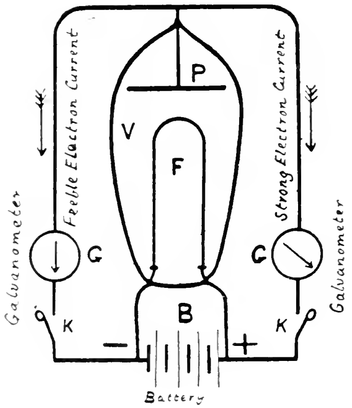

Fig. 1.—Scheme of Circuits for Exhibiting the Edison Effect.

Sir William Preece, and the latter found that the current called the Edison effect current increased very rapidly as the filament was heated to higher and higher temperatures, and that the collecting plate could be placed a long way from the filament, even at the end of a side tube, without altogether causing it to vanish.

At a little later date I took up the subject, convinced that there was yet much to learn about it, and one of the first things discovered was that the Edison effect was greatly reduced if that side of the carbon loop filament in connection with the negative pole of the battery was enclosed in a glass or metal tube, or if a sheet of mica was interposed between the filament and the collecting plate. This seemed to indicate that the effect was due to some material emission from the hot filament.

Another fact I observed very soon was, that the filament was giving off torrents of negative electricity, and could discharge a positively electrified conductor connected to the plate, but not a negatively charged one.

This is easily shown by connecting a gold leaf electroscope to the collecting plate, and charging it alternately with negative and with positive electricity when the filament is not alight. On making the filament incandescent it instantly discharges the positively charged electroscope, but not if it is negatively electrified. Furthermore, I found that the vacuous space between the filament and the plate possessed a curious unilateral electric conductivity for low voltage direct electric currents, and that even a single cell of a battery could pass a current from the hot filament to the collecting plate if the negative pole of the battery was in connection with the hot filament, but not in the opposite direction. This fact had, however, been previously noticed in another manner by W. Hittorf. These experiments were made in 1888 or 1889, and at that time were not satisfactorily explained.

It was not until nearly ten years later that your distinguished Professor of Natural Philosophy, Sir Joseph Thomson, published accounts of his epoch-making and important researches in which he proved that the agency we call negative electricity is atomic in structure, and exists in indivisible units now named electrons, which carry a certain electric charge and have a certain mass. This small natural unit of electricity is such that the quantity we reckon as one coulomb, viz., that which one ampere conveys in one second through any section of a conductor, is equal to $6\frac{1}{4}$ trillion electrons ($= 6 \cdot 25 \times 10^{18}$). These negative electrons are constituents of all chemical atoms. An electrically neutral atom which has lost one or more electrons is called a positive ion, and neutral atoms which have lost or gained electrons are said to be ionised. There are arguments in favour of the view that the majority of the atoms in metals and other good conductors of electricity are in a state of intermittent ionisation, and that intermingled with the atoms or positive ions, say in a wire of copper, tungsten or carbon, there are electrons which are jumping from atom to atom with great velocity. If we apply to the wire an electromotive force this causes a drift of these electrons at the instant they are free, in the opposite direction to the force (on usual conventions), which drift or unidirectional motion is superimposed on the irregular motion and constitutes an electric current. The drift velocity may be very slow compared with the velocity of the irregular motion. The drift motion of the electrons superimposed on the irregular motion may be compared with that of a swarm of bees in which each insect is flying hither and thither rapidly, whilst the whole swarm is being blown by a gentle breeze slowly down a road.

If the electrons merely surge to and fro, it gives rise to a form of

current we describe as an alternating current, and if they execute this motion very rapidly we call it an electric oscillation.

The reason an electric current produces heat in a conductor is because the drift energy of the electrons is then being continually converted into additional irregular-motion energy in the free electrons and atoms by collisions of electrons with the atoms of the conductor. If, then, the temperature becomes very high, that is, if the irregular electronic motion becomes very great, certain electrons may acquire such velocities that they are flung out from the surface of the wire even against the attraction of the positive atomic ions left behind. If there is no electric force tending to make the electrons move away from the neighbourhood of the hot wire these electrons constitute a *space charge* around it, and the repulsion they exercise on each other tends to keep other electrons from getting out into the space. Suppose, however, that the incandescent wire is placed in the axis of a highly exhausted glass tube, and is surrounded by a metal cylinder which is kept positively electrified, the electrons move to it, and others then make their exit from the wire. Such a tube with incandescent wire cathode and cold metal plate anode is now called a *thermionic tube*. The steady emission of electrons is called a *thermionic current*. In the case of a tungsten wire brilliantly incandescent in vacuo and under sufficient electric force, this current may amount to as much as an ampere per square centimetre of surface. This means that electrons are being flung or pulled out at the rate of millions of billions per second per square centimetre. As soon as Sir Joseph Thomson had proved by experiment that this electronic emission was taking place, the explanation of the effects observed in incandescent electric lamps by Edison, Preece and myself became clear. For in the Edison experiment we have a slow drift of electrons through the carbon filament superimposed on a very rapid and erratic motion, and multitudes of these electrons are escaping from the filament on all sides—just like steam escaping from a porous or leaky canvas steam pipe. If the plate in the bulb is connected to the positive pole of the filament-heating battery, it is positively electrified and it attracts these escaped electrons, and they enter it and drift through the external wire, forming the observed Edison current.

Suppose, then, that we connect the collecting plate by a wire external to the bulb with the negative terminal of the filament, and that we insert in this circuit a battery of a number of cells which can be altered so as to vary the potential of the plate, the said battery having its negative terminal connected to the filament, we then find that a thermionic current flows which can be measured by an amperemeter inserted in the circuit. If we vary the voltage from zero upwards we shall find that the thermionic current increases, but not indefinitely. It soon reaches a value at which no further increase of voltage raises the current. If we plot our observations

in a curve we obtain a so-called *characteristic curve* (see Fig. 2). The reason the current does not increase indefinitely is because for each particular temperature of the filament there is a certain maximum possible rate of electronic emission. The electrons are drawn away from the filament at a rate which increases with the potential of the plate up to that point at which the maximum emission rate is reached. The thermionic current then becomes stationary and is said to be *saturated*.

There is a definite relation between this saturation current reckoned in number of electrons escaping per square centimetre per second and the absolute temperature of the wire. Professor Richardson has deduced from certain theoretical considerations two formulæ which express this connection within certain limits of

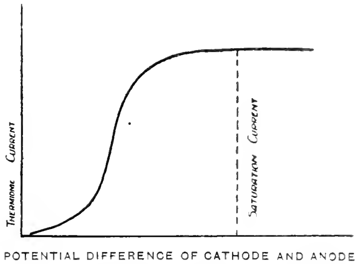

FIG. 2.—CHARACTERISTIC CURVE OF A THERMIONIC TUBE, HIGHLY EXHAUSTED.

temperature, and give nearly the same numerical result when compared with experiment. They are as follows :—

$$N = A\sqrt{T}\,\epsilon^{-\frac{b}{T}}$$

$$N = B\,T^2\,\epsilon^{-\frac{d}{T}}$$

where T is the absolute temperature, ϵ the base of Napierian logarithms, viz. 2·718 . . . , and A, B, b, d are certain constants peculiar to each substance.

For tungsten, Irving Langmuir found

$A = 1{\cdot}55 \times 10^{26}$ $\qquad b = 5{\cdot}25 \times 10^4$

Taking, for instance, $T = 2500°$ absolute, we find $N = 8 \times 10^{18}$, which is about 1 ampere per square centimetre per second.

These figures show the enormous number of electrons which may be projected per second from quite a small filament of highly incandescent tungsten in a high vacuum.

The formula also shows that this thermionic current increases very rapidly with the temperature. At a constant temperature and in a very high vacuum the thermionic current increases as the 1·5 power of the potential difference of the anode and hot cathode.

It is remarkable that although this emission of electricity from incandescent substances had been studied by leading physicists for more than a quarter of a century, none of them (not even Mr. Edison) made any practical application of it prior to 1904 in telegraphy or telephony, or indeed in any other way. At that date I was so fortunate as to discover a new and totally unexpected application of this thermionic emission in wireless telegraphy. By that time wireless telegraphy by electromagnetic waves had been brought into a condition of considerable utility, chiefly by the work of Senator Marconi and his collaborators.

I shall assume that the broad general principles of it are familiar to all present, and that it is understood that this kind of telegraphy is accomplished by electromagnetic waves, which differ only from ordinary light in that their wave-length is reckoned in hundreds or thousands of feet rather than fractions of an inch. The waves sent out from the great Carnarvon Wireless Station, for instance, have a wave-length of 50,000 feet, whereas yellow-green light has a wave-length of $\frac{1}{50000}$th part of an inch.

When these long electric waves strike an elevated ærial wire they create in it very feeble alternating electric currents, and these have to be appreciated by sensitive instruments called *detectors*.

Before 1904 only three kinds of detector were in practical use in wireless telegraphy—viz. the coherer, or metallic filings detector, the magnetic-wire detector, and the electrolytic detector. The first operated in virtue of the fact that feeble electrical oscillations can alter the electric conductivity of collections of metallic particles. The second depended upon the power of electric oscillations to shake up the molecules or groups of molecules of an iron wire and promote magnetisation or demagnetisation; and the third by reason of the fact that rapid alternating currents can improve the contact between a small platinum wire electrode and an acid electrolyte in which it is immersed and by which a small direct current is passing into the liquid.

The coherer and the electrolytic detectors were both rather troublesome to work with on account of the frequent adjustments required. The magnetic detector was far more satisfactory, and in the form given to it by Senator Marconi is still used. It is not, however, very sensitive, and it requires attention at frequent intervals to wind up the clockwork which drives the moving iron wire band.

In or about 1904 many wireless telegraphists were seeking for new and improved detectors.

I was anxious to find one which, while more sensitive and less

capricious than the coherer, could be used to record the signals by optical means, and also for a personal reason I wished to find one which would appeal to the eye and not the ear only through the telephone. Our electrical instruments for detecting feeble direct or unidirectional currents are vastly more sensitive than any we have for detecting alternating currents. Hence it seemed to me that we should gain a great advantage if we could convert the feeble alternating currents in a wireless aerial into unidirectional currents which could then affect a mirror galvanometer, or the more sensitive Einthoven galvanometer. There were already in existence appliances for effecting this conversion when the alternations or frequency was low—namely, one hundred, or a few hundred per second.

For example, if a plate of aluminium and one of carbon are placed in a solution of sodic phosphate, this electrolytic cell permits positive electricity to flow through it from the aluminium to the carbon, but not in the opposite direction. The reason is because the electrolysis covers the aluminium with an impervious film of hydroxide in the first case and reduces this film in the second case. But such electrolytic rectifiers, as they are called, are not effective for high frequency currents, because the chemical actions on which the rectification depends take time. After trying numerous devices my old experiments on the Edison effect came to mind, and the question arose whether a lamp with incandescent filament and metal collecting plate would not provide what was required even for extra high frequency currents, in virtue of the fact that the thermionic emission would discharge the collecting plate instantly when positively electrified, but not when negatively. Accordingly I appealed to the arbitrament of experiment, and the following arrangement was tried.

Two coils of wire were placed at a distance, and in one of them electric oscillations were created by the discharge of a Leyden jar. The other coil had one terminal connected to the filament of a lamp, and the collecting plate to one terminal of a galvanometer, the second terminal of the latter being connected to the second terminal of the coil.

I found to my delight that my anticipations were correct, and that electric oscillations created in the second coil by induction from the first were rectified or converted into unidirectional gushes of electricity which acted upon and deflected the galvanometer, as now shown.

I therefore named such a lamp with collecting metal plate used for the above purpose, an *oscillation valve*, because it acts towards electric currents as a valve in a water-pipe acts towards a current of water. I soon found that for the purposes of wireless telegraphy quite a small low voltage lamp with a metal cylinder placed round a carbon or metal loop filament was a very effective rectifier, and could be used for converting the feeble alternating currents in a wireless receiving aerial into unidirectional currents capable of affecting a

telephone or galvanometer. It was almost immediately adopted in practical wireless telegraphy as a simple and easily managed detector, and the intermittent rectified currents were passed through a telephone (Fig. 3).*

It may be just as well to mention here that there are two systems of wave generation in use in wireless telegraphy. In one the waves are created by powerful intermittent condenser discharges. These

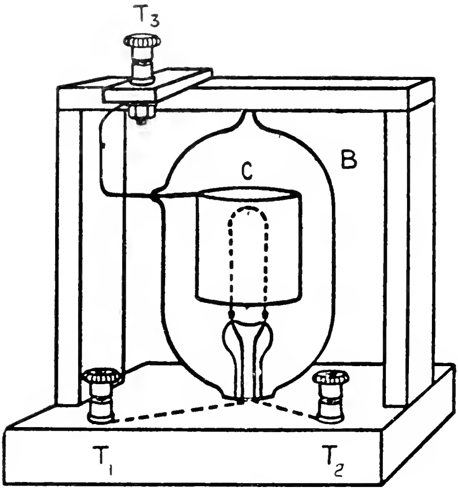

Fig. 3.—Fleming Oscillation Valve, on Stand.

C, metal cylinder surrounding the filament of a glow lamp, and connected to a terminal T_3. B, glass bulb highly exhausted. T_1 T_2, terminals of the lamp filament.

waves are radiated in groups of about twenty to fifty waves, and 300 to 500 such groups are sent out per second. The receiving aerial

* For the purposes of patent litigation and controversy it has frequently been urged that there was little or no invention involved in thus applying an incandescent lamp with a metal cylinder in the bulb as a detector for wireless waves, seeing that such an appliance had already been made by Mr. Edison. Important legal judgments confirmed by the United States Court of Appeals have, however, held that it *did* constitute invention, and of a very meritorious device: and that, prior to the disclosure by me, it was not known to men skilled in the radio art that such a device would rectify and detect oscillations of high frequency as used in wireless telegraphy. The invention therefore consists not in the appliance in itself, but in the altogether new, unexpected, and highly useful application made of it by me in radiotelegraphy, which opened a vast field for subsequent invention based on the same facts of thermionic emission.

then picks them up and the valve rectifies the groups into unidirectional gushes of electricity. These passing through the telephone give rise to a steady musical sound. By radiating short or long collections of groups of waves, the sound in the telephone can be cut up into short or long periods, which make the dot and dash signals of the Morse alphabet.

In the other system, called the continuous wave (C.W.) system, the waves are sent out in an unbroken stream, except in so far as this stream is cut up into short and long periods to make the Morse signals. The method by which these continuous waves are detected will be explained presently.

Some time after the introduction of this oscillation valve I found that another method of employing it as a detector was as follows :—

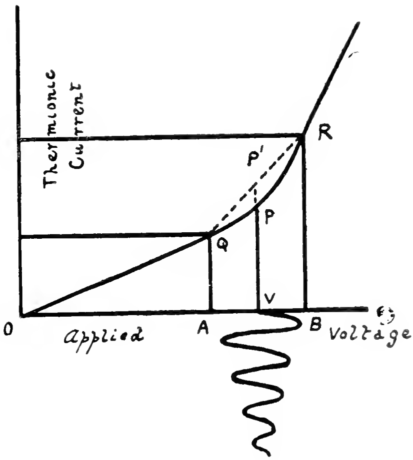

Fig. 4.—Change of Slope of Characteristic Curve.

If we connect the plate of the valve with the negative terminal of the filament heating battery, and insert in that circuit a battery for creating a thermionic current, we can delineate a characteristic curve, as already described, by varying the electromotive force (E.M.F.) of the plate circuit battery. That curve has generally some places in it at which the slope changes rather quickly. If we adjust the E.M.F. of the plate battery to work at that point, and then by means of a transformer superimpose a feeble oscillatory E.M.F. derived from a wireless receiving aerial, the thermionic current will oscillate from one value to another, and it is easy to see from the concave form of the characteristic curve that the mean value of this varying thermionic current is greater than the value of the steady thermionic current when the oscillations are not superimposed on the steady or battery voltage (Fig. 4). This

mode of usage in the case of valves with a certain degree of exhaustion in the bulb gives very great sensitiveness in the detection of radio-signals. It is commonly called the potentiometer method because the extra steady voltage required in the plate circuit is derived by employing a fraction of the voltage of the battery used for incandescing the filament by means of a potentiometer resistance. This is, perhaps, the place to refer to another view of the mode in which my valve acts even when no additional E.M.F. is placed in the plate circuit. On delineating, as above described, the characteristic curve of a valve, it is found that this curve does not start exactly from the point of zero voltage, but from a point on the negative side about $\frac{3}{4}$ to 1 volt (Fig. 5). This means that if the

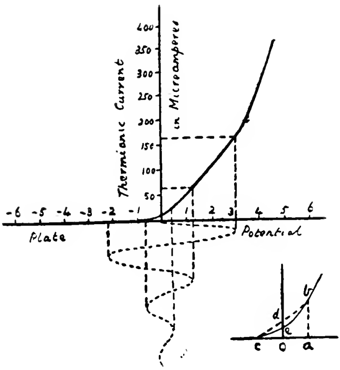

Fig. 5.—Characteristic Curve near Origin.

plate is connected to the negative terminal of the filament battery by a wire, there is found to be in it a small negative electric current flowing from the plate through the external circuit to the negative terminal. At first sight it seems to imply that the electrons return back on their own origin, but the true reason probably is that the electrons are shot out of the filament with a certain velocity and accumulate round the plate. The result is a tendency for them to diffuse back through the external circuit, creating a feeble electron current which can only be stopped by introducing a small counter E.M.F. into that circuit.

Hence the characteristic curve starts from a negative point on the voltage axis. At the place where it crosses the zero voltage point that curve is concave upwards, and hence, for the reason just explained, the introduction into the external thermionic circuit of a feeble alternating high frequency electromotive force will result in an increase in the mean or average thermionic current. Hence the

valve is sensitive to feeble electric oscillations and rectifies them, not by quite suppressing all current in one direction, but because the thermionic current is greater for a given E.M.F. applied in one direction in the thermionic current than when that E.M.F. is applied in the opposite direction, whilst the mean value of the thermionic current throughout the complete cycle is greater than its value when the alternating E.M.F. is not applied.

We must now turn to consider an improvement which was introduced in 1907 into the thermionic value, for which credit must be given to Dr. Lee de Forest. He placed a grid or zig-zag of wire

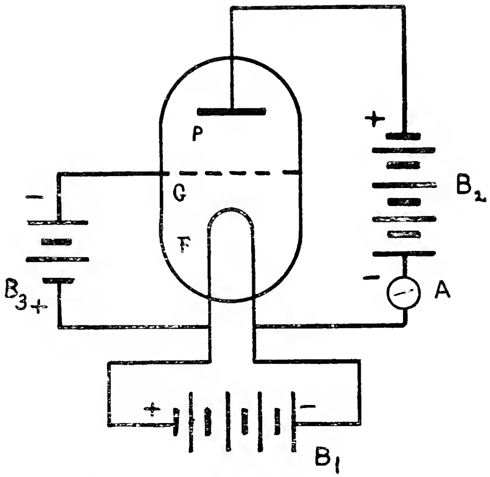

FIG. 6.—CONVENTIONAL DIAGRAM OF A THREE-ELECTRODE VALVE.

P, a metal-plate or cylinder in a highly exhausted glass bulb. G, a grid or perforated plate or spiral wire. F, the lamp filament. B_1, the filament heating battery.

carried on a separate leading-in wire between the plate and the filament of my valve, and thereby made what is now called a three-electrode valve (Fig. 6).

In modern thermionic devices the grid takes the form either of a spiral wire or else a metallic gauze cylinder, which surrounds the filament without touching it, and is in turn surrounded by the plate or cylinder which does not touch the grid (Fig. 7). This addition enables the valve to act as an amplifier of electric oscillations as follows:—

Suppose we insert in the external plate circuit a battery B_2 (see Fig. 6) giving an E.M.F., say, of 100 volts, and also a current-measuring instrument A. If the battery has its positive terminal

connected to the plate, the stream of electrons emitted by the filament will be drawn to the plate and give a thermionic current of three or four milliamperes if the valve is highly exhausted.

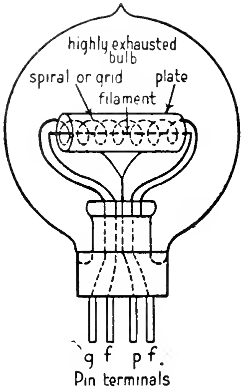

Fig. 7.—A Modern Three-Electrode "Hard" Valve of a Type Usually Called the French Valve.

This stream of electrons will reach the plate by shooting through the holes or interspaces in the mesh or spiral grid G.

Let us now suppose that we give the grid a small negative charge by a battery B_3. This will cause the electrons coming out of the

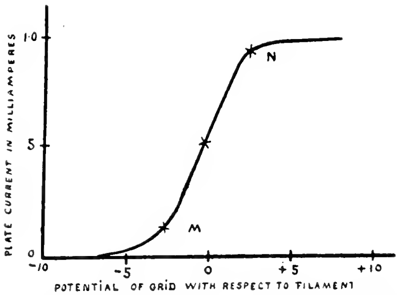

Fig. 8.—Characteristic Curve of a Three-Electrode Valve.

filament to be partly repelled, and therefore the thermionic current in the plate circuit will be reduced perhaps even to zero.

Again, let us give the grid G a small positive charge. This will attract the emitted electrons and they will shoot through the grid

with increased velocity. Therefore the thermionic current will be increased. The important point to notice is that, owing to the small electrical capacity of the grid, and also owing to the high voltage acting in the plate circuit, a very small expenditure of power on the grid circuit will vary or modulate a much larger amount of power in the plate circuit. Just as the pressure of a child's finger on a switch may start or stop an electric motor of several horse-power, or a feeble current passing through a telegraph relay start or stop a large current, so the three-electrode valve acts as a relay.

If we plot a curve delineating the variation of thermionic current with varying grid voltage or potential for such a three-electrode valve we find that curve over wide limits to be nearly a straight line (Fig. 8). This means that the change in plate current is proportional to the change in grid voltage.

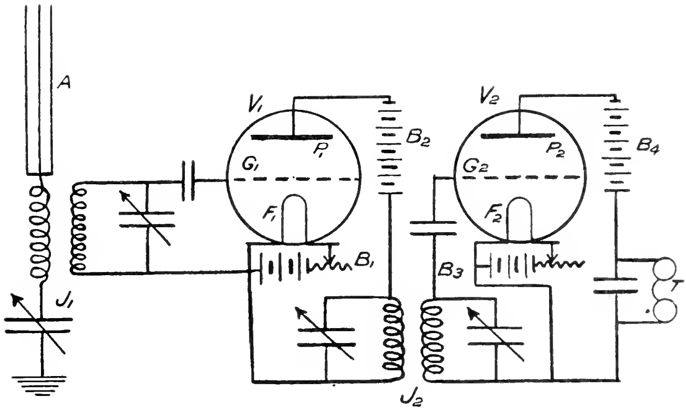

FIG. 9.—TWO THERMIONIC VALVES COUPLED IN CASCADE.

However rapidly the grid voltage may change, so nimble are these little electrons that the thermionic current copies on a magnified scale the changes of grid potential. Hence the arrangement is called a thermionic amplifier.

We can, however, advance further. If we cause the plate current of one valve to pass through the primary coil of a transformer, and then connect the terminals of the secondary coil of the latter respectively to the grid and filament of a second valve, we find that the fluctuations in the plate current of the first valve can be made to generate exalted potential variations of the second valve, and this again to create magnified variations of the plate current of the second valve (see Fig. 9).

This mode of connection is not limited to two valves; we can thus employ three, four or more valves *in cascade*, as it is called, and

each one multiplies or amplifies the effect of the one before. Thus, if the first valve multiplies potential variations ten times, or has an amplification factor of ten, then two in cascade amplify 100 times, and three 1000 times, and so on (Fig. 10). It is this use of three-electrode valves in cascade that has given us recently such vastly increased powers of detecting wireless waves.

The last or final amplifying valve may be made to operate a detecting or rectifying valve, or perhaps a crystal detector.

I have on the table a set of such cascade valves, six in series, with one detector valve, which has been kindly lent by the Marconi Company, and is used in their radio-telegraphic work. Fig. 16 shows the external appearance of this receiver, and Fig. 10 the diagram of connections.

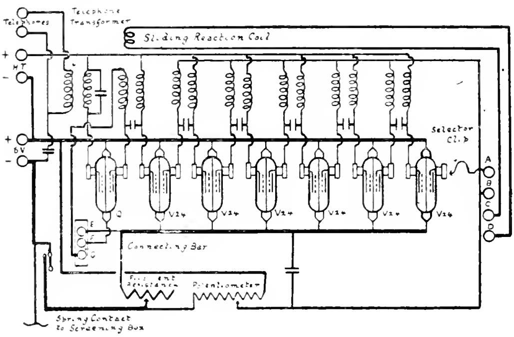

Fig. 10.—Marconi Company's Valve Detector with Valves in Cascade, Type 55.

But there is an additional very valuable power possessed by the thermionic valve, viz. that it can generate electric oscillations as well as detect them. This property is common to the two- and also to the three-electrode valves, but as time is limited I shall confine my explanations to the mode in which the latter operates as a generator of oscillations.

We have already seen that the fundamental property of this valve is that variations of grid potential create similar variations of plate or thermionic current. Supposing, then, that this latter current is passed through a coil over which is wound another secondary coil which connects the grid and filament (Fig. 11). It is possible so to make the connections that any increase in the plate current will give the grid a negative charge and so immediately reduce the plate current. Conversely, any reduction of plate current will give the grid a positive charge which will again increase the plate current. Hence the operations in the plate current when once started will be maintained, the energy required being drawn from the battery B (see

Fig. 11) in the plate circuit. The action resembles that in the well-known experiment called the singing telephone. If we place an ordinary carbon microphone transmitter in series with a battery and receiving telephone, both having diaphragms of equal size, and hold the receiver mouth towards the transmitter mouthpiece, the receiver will emit a shrill musical note.

The reason is not far to seek. Small noises in the room make little vibrations in the diaphragm of the transmitter. These vary the carbon resistance, and therefore the current flowing through the receiver. The latter then emits a sound, and as the natural rate of vibration of the diaphragm of the transmitter is the same, this sound maintains the transmitter in vibration. The two act and re-act on

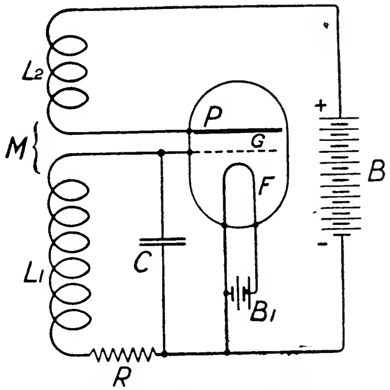

Fig. 11.—Connections for Generator Valve.

each other, and the energy required to produce the sound is drawn from the battery.

Another illustration which assists in explaining the action of the oscillating valve is that of the steam engine. The steam is admitted first at one end of the cylinder and then at the other to cause a reciprocating motion in the piston. The agent for effecting this distribution of the steam is the slide valve, which admits the steam alternately above or below the piston. But the piston itself moves the slide valve by the intermediation of the eccentric or in some other appliance, and so the engine becomes self-acting and the steady pressure of the steam maintains a to-and-fro motion in the piston rod.

The potential of the grid of the valve answers to the slide valve, the plate current to the piston, and the steady E.M.F. of the plate battery to the steam pressure, whilst the alternating current set up in the plate circuit corresponds to the reciprocating motion of the piston.

The discovery of the oscillation-producing power of the valve was of great importance, because it at once put it in our power to conduct wireless telephony with simple easily-managed apparatus. The principles of radio-telephony are briefly as follows: At the transmitting station we have to establish in the sending aerial undamped or persistent oscillations. and to radiate continuous waves. This corresponds to a lighthouse which is emitting a steady persistent beam of light. By means of a carbon microphone we have then to modulate the amplitude or intensity of these waves in accordance with the wave form of the speaking voice. Any articulate sound we can utter is built up of syllables, each of which generally comprises a certain vowel-sound with a more or less abrupt beginning or ending due to the consonant. The vowel sound is a series of sound-waves

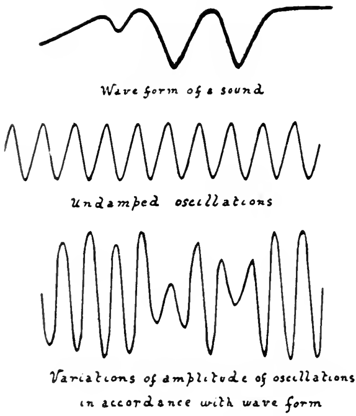

Fig. 12.—Variation of Amplitude of Continuous Waves in accordance with Speech Wave Form.

which at any point through which they pass create compressions or rarefactions in the air, and the mode in which these states vary with time may be represented by the ordinates of a curve called the wave form curve, the abscissæ denoting the corresponding time.

These sound-waves acting on the diaphragm of a carbon microphone transmitter are caused to produce similar variations in the electric current flowing through it.

The arrangements for a wireless telephone transmitter are, then, as follows: By means of a thermionic valve, with its plate and grid circuit inductively coupled, we set up, as already explained, persistent electric oscillations in the plate circuit, and these are transferred by induction to an aerial wire properly tuned to sympathetic vibration. High frequency electric currents therefore flow up and down the aerial. These produce magnetic and electric effects in surrounding

space which are propagated outwards as an electromagnetic wave. Whether, in view of Einstein's revolutionary theory of Relativity, we have justification for speaking any longer of an electromagnetic medium or of an æther must be left to those who have more knowledge of that theory than your lecturer possesses, but we may, perhaps, still be permitted to postulate a space-filling medium as a convenient hypothesis. It is certain that wireless telephony is achieved by some kind of undulation propagated through space, and if so, then there must surely be something which undulates.

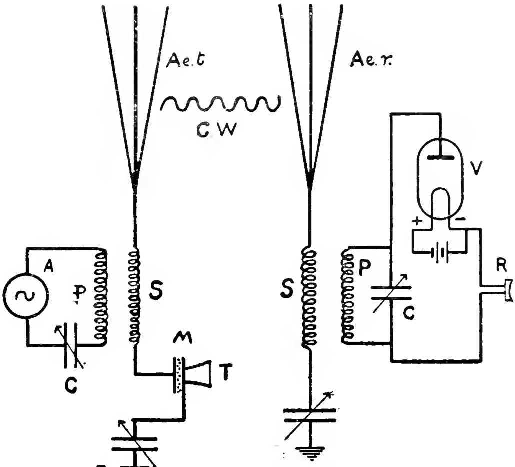

Fig. 13.—Diagrammatic Sketch showing the Principles of a Wireless Telephone Transmitter and Receiver.

Ae.t, transmitting aerial. Ae.r, receiving aerial. A, a high frequency alternator or else a generating valve. P, S, oscillation transformer. T, speaking carbon microphone. V, receiving valve. R, receiving telephone.

We have in the next place to vary the amplitude of these radiated electromagnetic waves by a speaking microphone, and this is done by means of a Control Valve.

This latter valve has its grid circuit inductively connected by a transformer with a circuit containing a battery and a telephone transmitter.

Hence, when speech is made to the mouthpiece of the carbon microphone, this varies the electric current through it, and therefore the potential of the grid, in accordance with the wave form of the speech sound. The plate circuit of this control valve is joined in parallel with that of the generating or power valve, and the result is

that speaking to the carbon transmitter modulates the amplitude of the aerial current, and therefore the amplitude of the radiated waves in accordance with the speech wave form (Fig. 12).

At the receiving station these electromagnetic waves impinge on the receiving aerial and create in it very feeble alternating currents, which are a copy on a reduced scale of those in the transmitting aerial. These are then amplified by valves in cascade, rectified, and sent through a Bell receiving telephone. The result is that the latter emits sounds which closely imitate the speech sounds made to the distant transmitter (see Fig. 13).

Although this all seems extremely complicated when described in words, it really works very well in practice. Certain things however are conditions of success. In the first place a generating valve must be a "hard" or high vacuum valve, for if it is "soft" or low vacuum

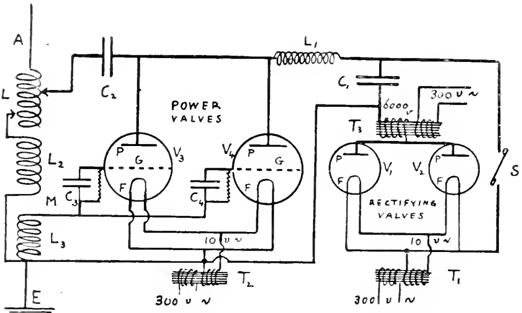

FIG. 14.—GENERATOR VALVE SET FOR WIRELESS TELEPHONY.

the electron emission from the filament will ionize the residual air and the positive ions will bombard the filament and soon destroy it. Again, we require very high E.M.F. to create a thermionic current of sufficient strength for wireless telephony. This is now obtained by rectifying a high voltage low frequency alternating current by a Fleming two-electrode valve. In practice a small alternator is employed, and by means of step-down transformers a low voltage current is provided for heating the filaments of the three valves (see Fig. 14). A step-up transformer working off the same alternator furnishes a high voltage current which is rectified by a valve, and this gives us the unidirectional plate voltage required for the generating and control valves (Fig. 13).

The whole of the appliances are usually contained in a small cabinet, and the Marconi Company have kindly sent one of their ¼-kilowatt radio-telephone sets for exhibition (Fig. 15).

Such a transmitter will work over 200 miles, as from Chelmsford to Amsterdam, and transmit speech perfectly. More powerful

arrangements on the same principle have telephoned from Chelmsford to Rome.

For aircraft radio-telephony it is usual to provide a small high-tension dynamo driven by a wind-screw, which is fixed to some external part of the fuselage, and this gives the requisite direct high plate voltage for the generating valve. The filament-heating currents for the generating, the control and the receiving valves are provided from small closed storage cells. The aerial wire is a long trailing wire about 250 feet in length, which is unwound when required from a drum.

The principles of operation are as above described, and the engineers of the Marconi Company have worked out an ingenious arrangement by which the actual valve apparatus may be placed at

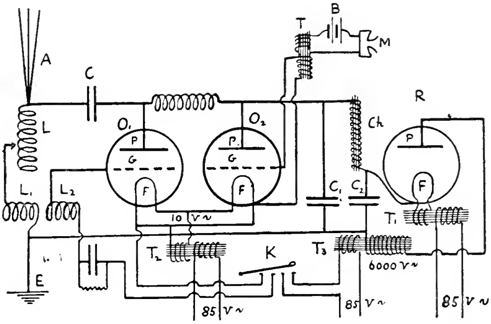

FIG. 15.—CIRCUIT CONNECTIONS OF A MARCONI WIRELESS TELEPHONE TRANSMITTER.

O_1, the generating valve. O_2, the control valve. R, the rectifying valve.

any convenient place in the aeroplane body and yet be controlled by the pilot or observer from their seats. The mere act of taking hold of the microphone transmitter closes a switch which lights up the valves and throws over the aerial wire into connection with the transmitting valve.

The pilot has also in front of him a small ammeter which shows at a glance if the proper high-frequency oscillations are being generated in the aerial wire. Such aircraft radio-telephones will operate over a distance of 50 miles or more. So sensitive are these cascaded valve detectors that it is not even necessary to use a long aerial wire at all. A very few turns of insulated wire wound on a wooden frame, called a frame aerial, connected to the receiver suffice to collect and detect the electric wave signals.

Experiments were conducted in March, 1919, by the Marconi Company to ascertain the minimum power required by these valve

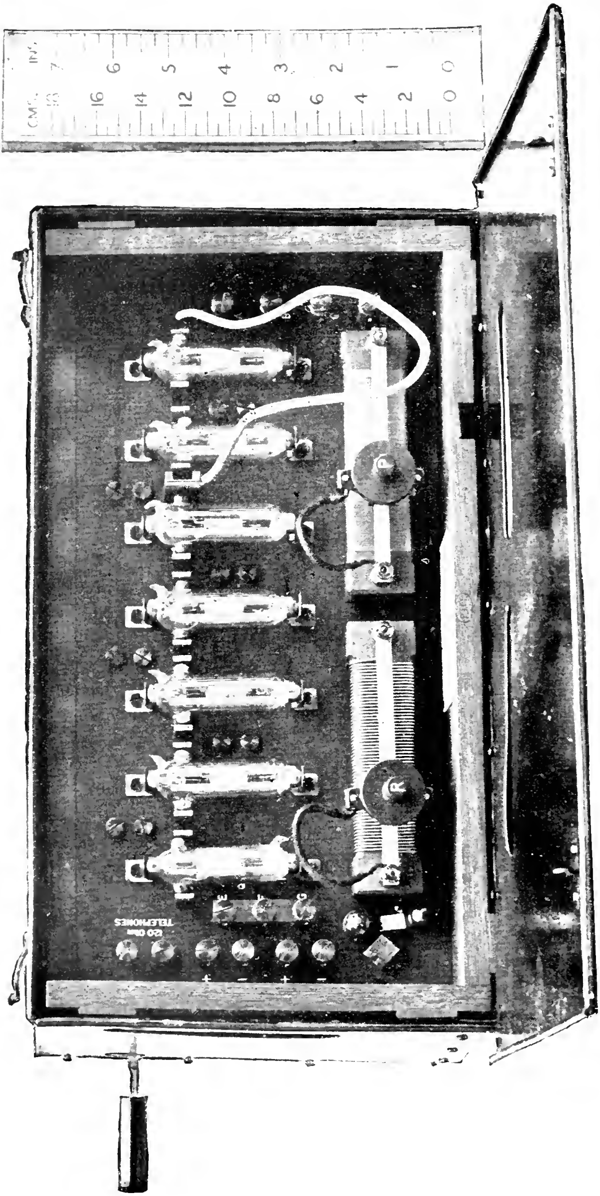

Fig. 16.—Marconi Company's Multiple Valve Receiver, with Six Valves in Cascade and One Detector Valve.

generators to transmit articulate speech across the Atlantic during daylight hours. These experiments were conducted between Ballybunion, Co. Kerry, Ireland, and Louisberg, Cape Breton, Nova Scotia. The engineers in charge responsible for the design and working of the stations were Mr. W. T. Ditcham in Ireland and Mr. W. J. R. Picken in Nova Scotia. The tests lasted over 10 or 12 days, and were all carried out between 10 a.m. and 1 p.m. G.M.T. The distance separating the stations is 1800 miles.

The aerial wire was of umbrella form, supported on masts 500 feet high. The transmitting plant consisted of two three-electrode generating valves, with a third control valve for speech modulation. A small alternator of 2·5 kilowatt power supplied an alternating current which was stepped up in potential to 12,000 volts and rectified by a two-electrode or Fleming valve. The continuous waves generated had a wave length of 3800 metres, or nearly 2½ miles. The receiving appliance comprised a multiple valve detector with valves in cascade. The external appearance is as shown in Fig. 16, and the connections as shown in Fig. 10.

The reception was by a series of six valves in cascade, with a final detector valve. The speech transmission was perfectly good and clear across the Atlantic, and so loud at Chelmsford, 500 miles away from Ballybunion, that it could be heard on a simple frame aerial. There was nothing of the nature of freak transmisssion, but for regular working at all times experience showed that rather more power will be required. A battery of half-a-dozen generator valves of the type used, and now exhibited here, will probably be sufficient to give a regular service of trans-atlantic radio-telephony.

Before leaving the subject of radio-telephony it may be remarked that, both in connection with it and with the every-day uses of radio-telegraphy in maritime intercommunication, there is a great demand for an effective wireless bell call. On board ship where a message or even an S.O.S. signal may come in at any moment, the operator on duty has to sit with the telephones to his ears, held there by a steel head-spring.

Ever since the terrible tragedy of the s.s. "Titanic" it has been the custom for large ships to carry two operators, who take wireless duty in turn, but in small ships where only one operator is carried the duties may become very onerous, or some important call may be missed.

In ordinary exchange telephony we call up the exchange or a subscriber by ringing an electric bell. To do this, however, requires more power than can be supplied directly from the receiving aerial. Some sensitive form of relay has to be employed in which the aerial current closes the circuit of a local battery, and this supplies a current which rings a bell. In wireless work, however, certain conditions have to be fulfilled. It is essential that the relay should not be put in action by every passing electric wave, or dot-and-dash message

signal travelling through the æther. The plan usually adopted is to give the relay so much inertia that the circuit is only closed by sending signals composed of several very long dashes or trains of electric waves.

I have recently devised a form of bell-call which depends upon the use of a new type of four-electrode valve, made as follows: A highly exhausted glass bulb contains a straight filament of tungsten, which is rendered incandescent by a 6-volt battery. Around the filament are arranged four narrow curved metal plates, having their curved sides facing the filament and very near to it. Each of these plates is carried on a wire sealed through the glass bulb. The plates are arranged round the filament, as shown in plan in Fig. 17.

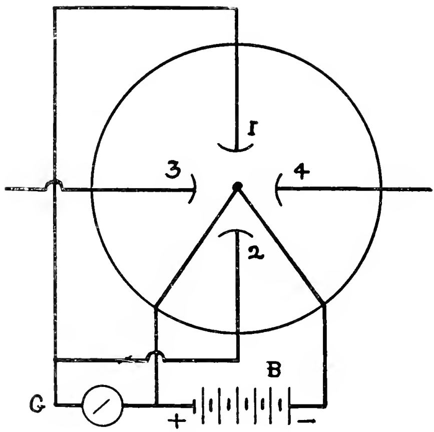

Fig. 17.—Fleming Four-Anode Valve.

1 and 2 are the collecting plates. 3 and 4 are the potential or deflecting plates. B is the filament-heating battery, and the central dot is the end-on view of the straight filament. G is a relay or galvanometer.

Two of these plates on opposite sides of the filament, viz. 3 and 4 (see Fig. 17), are called the potential plates, and the other two the collecting plates. The collecting plates are joined together outside the bulb and connected to the positive terminal of the filament-heating battery, and a galvanometer G or telegraphic relay inserted in that circuit. The electronic emission from the filament then creates a current which flows through the galvanometer or the relay, as in the Edison experiment. If the two other plates have a small potential difference made between them, either of constant direction or else a high frequency alternating difference, this suddenly reduces the thermionic current. The potential difference of the potential plates introduces a new electric force into the field which

deflects away the electrons proceeding from the filament and prevents them reaching the collecting plate. If, then, we connect the potential plates to the ends of a resistance of about 15,000 or 20,000 ohms, and include this resistance in the plate circuit of an ordinary three-electrode valve, the thermionic current of the latter flowing through the resistance will create a terminal potential difference which arrests the thermionic current of my new valve. Hence the relay does not operate. If, however, we give an extremely small negative potential to the grid of the three-electrode valve, then this reduces the thermionic current of the latter and increases that of the other valve, which again in turn causes the relay to close contact, and it may be thereby caused to ring a bell. The negative grid

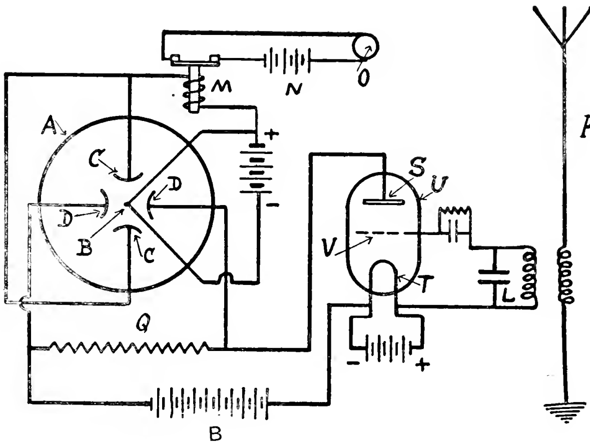

FIG. 18.—ARRANGEMENTS FOR A WIRELESS ELECTRIC BELL CALL EMPLOYING A FOUR-ANODE VALVE.

potential can be derived from the oscillations in an aerial wire as above described. In this manner I have constructed an arrangement by which the ordinary feeble antenna oscillations can be employed to ring a call-bell (see Fig. 18). The arrangement constitutes an effective form of wireless electric bell. The operator can then switch over the aerial to an ordinary valve receiving set and listen to the telephone.

The Marconi Company's engineers have worked out a type of call-bell apparatus in which the "needle" or movable part of a sensitive relay is made to close the circuit of a local battery and so ring an electric bell. The peculiarity of this relay is that the "needle" is not deflected sufficiently far to close contact by any ordinary message signals, but only by a prolonged series of "dots"

sent at the rate of 180 per minute. Even then the pressure at the surfaces to be brought in contact is so small that not sufficient local battery current can pass to ring the bell unless a plan devised by me for improving such relay contacts is employed. This consists in sending feeble electric oscillations across carbon-platinum contact surfaces.*

It then remains to say a few words on the methods by which the thermionic valve is employed in the reception of signals made by undamped or continuous waves. This system has for many reasons an increasing importance as compared with the damped or intermittent wave system. There are four methods in use for creating undamped waves, viz. :—

1. By a high frequency alternator, which is only an ordinary alternating current dynamo constructed to give currents which alternate 30,000 to 100,000 times a second instead of 25 to 100. Such machines require very exact construction and are expensive to build.

2. By a Poulsen arc, which is a large direct current electric arc, formed between a carbon and copper electrode placed in a strong transverse magnetic field and in an atmosphere of hydro-carbon vapour. The arc is shunted by a condenser in series with an inductance in which circuit high frequency oscillations are set up by the arc.

3. By a system invented by Senator Marconi, called the timed-spark, in which a series of condenser discharges are created in sequence and so timed that they produce trains of electric oscillations, one beginning at the instant the previous one ends and in step so as to produce in effect an undamped oscillation. This system is in operation at the great wireless Marconi station at Carnarvon.

4. Lastly, we have the method by thermionic valves already described.

By far the best method of receiving signals by these waves is by the so-called beat-reception.

If two sets of waves of slightly different wave length are super-imposed, no matter what sort of waves they be, the result is to produce a compound wave with periodically increasing and decreasing amplitude. These augmentations are called the *beats*.

If a continuous electric wave falls on an aerial it creates on it continuous oscillations. Suppose, then, that we generate also by some local means in the aerial wire undamped oscillations differing in frequency, say by 1000, from the incident waves. The result will be to produce in the aerial electrical beats having a frequency of 1000. These act to a receiver just as do damped trains of waves with a train frequency of 1000. They can be rectified and detected by a valve and telephone as already explained. When this method

* See British Patent, J. A. Fleming, No. 112544 of 1918.

was first suggested by R. A. Fessenden the only practical method of producing the local high frequency, but controlled, oscillations was

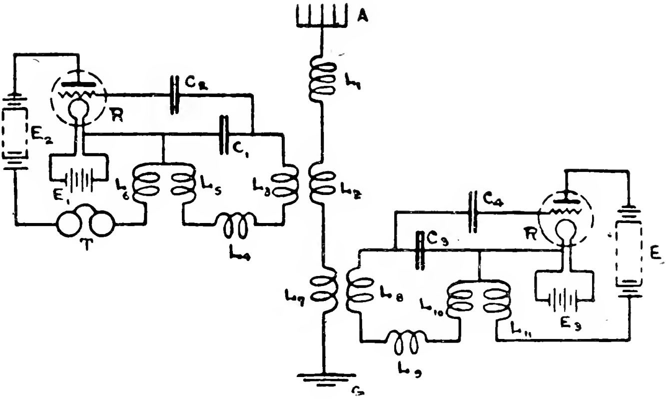

FIG. 19.—VALVE RECEIVER SET FOR C.W. WAVES, HETERODYNE RECEPTOR.

by the expensive high frequency alternator. The method was therefore not much used. But the discovery of the oscillation-producing power of the thermionic valve put a fresh face on matters. It now

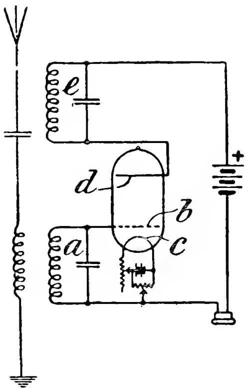

FIG. 20.—ROUND'S METHOD OF BEAT RECEPTION WITH A SINGLE THREE-ELECTRODE VALVE.

became quite easy to produce high frequency oscillations, of any required periodicity, by coupling a three-electrode valve to the aerial and then coupling the grid and plate circuits of the valve.

Sometimes a separate three-electrode valve is used to rectify and detect the beats (see Fig. 19).

Captain H. J. Round has, however, invented ingenious methods by which one and the same thermionic valve can be used to generate the beats and at the same time to detect them (see Fig 20).

There are a large variety of valve connections which have been devised for the same purpose, but time does not permit reference to them.*

We must in the last place glance at the uses of the thermionic valve in connection with ordinary telephony with wires.

When the rapid fluctuating electric currents flow along a copper telephone line, which are propagated when a speaker at one end of a long line converses by telephone with an auditor at the other, two effects take place which militate against clear and audible speech transmission. First, the current generally is enfeebled as it flows, and this is called the attenuation. Secondly, the different harmonic constituent currents which go to make up the complex wave form which corresponds to each articulate sound are differently enfeebled.

The vibrations of high pitch are more enfeebled than those of lower pitch. The first effect reduces the loudness of the speech received, and the second its articulate clearness or quality. The cause of the general enfeeblement is the resistance of the line, which fritters away the energy of the speech electric currents. Until lately the only known method of overcoming it was by putting sufficient copper into the line, but this of course means cost. Thus, for instance, the London to Glasgow telephone trunk line is a double copper conductor of thick wire weighing 1600 lb. per mile run. For the 400 miles or so to Glasgow this requires nearly 300 tons of copper, and the price of that at present is about £30,000 for mere copper.

This mass of copper is required to give sufficient loudness to the telephonic speech over 400 miles of transmission.

The thermionic valve is, however, able to make a very large economy in copper. It has already been explained that the three-electrode valve can act as an amplifier. Suppose then that we cut a long telephone line in the middle and insert on one side a transformer, the secondary terminals of which are connected to the grid and filament of a valve, whilst the plate circuit also contains a battery B_1 (see Fig. 21) and a transformer of which the secondary circuit is in connection with the continuation of the line. Feeble telephonic currents arriving at the valve would vary the potential of the grid, and this, as just explained, would fluctuate in like manner, but with increased energy, the plate current. The transformer in the plate circuit would then re-transmit the speech current, but with exalted amplitude.

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* The reader may be referred for additional information to a treatise on "The Thermionic Valve in Radiotelegraphy and Telephony," by J. A. Fleming, published by The Wireless Press, Ltd.

The valve thus can be used to counteract the effect of resistance on the line. In practice, however, the arrangements are a little more complicated, because a telephone line has to be used in both directions. This can be done, however, by means of an arrangement used by Mr. Edison for a similar purpose. We have to contrive matters so that telephonic currents arrive in either direction, and

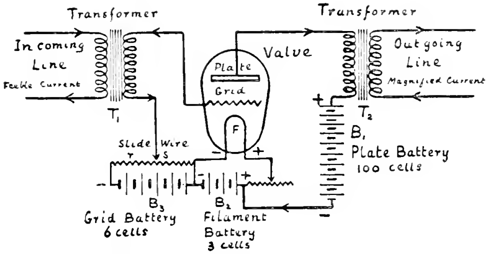


FIG. 21.—TELEPHONE THERMIONIC REPEATER.

yet to avoid letting the plate current of the valve re-act upon its own grid, which would set up a loud howling in the receiving telephones. The scheme of circuits for effecting this is shown in Fig. 22.

The simple arrangement shown in Fig. 22 would, however, only be suitable if the valve was placed exactly in the electrical centre of the line, and this is not always possible. In the actual arrangement

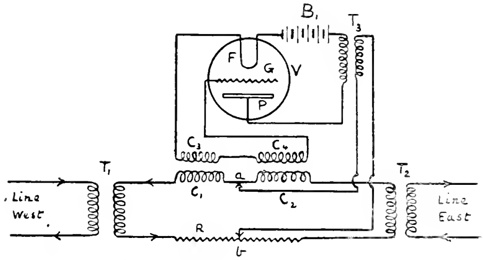


FIG. 22.—THERMIONIC REPEATER EMPLOYED WITH AN EDISON CIRCUIT IN TELEPHONY WITH WIRES.

each section of the line on either side of the valve is balanced against an equal artificial line, and a pair of valves are used, one of which operates for currents in one direction and the other for the reverse (Fig. 23).

By the use of such a thermionic repeater, as it is called, speech can be retransmitted with enhanced loudness, and a considerable

saving effected in the copper necessary for good speech over a certain distance.

The Engineering Department of the General Post Office are now thoroughly testing this thermionic repeater and using it in practice. The Engineer-in-Chief has kindly lent an experimental set by which you will be able to hear speech audibly transmitted over an artificial line equal to forty miles of standard cable, which is about the telephonic limit for that size of cable, and note that with the valve repeater the speech over forty miles is as good as the speech over twenty miles of the same cable without the valve. In other words, the valve doubles the range.

If our trunk telephone line system in Great Britain had to be

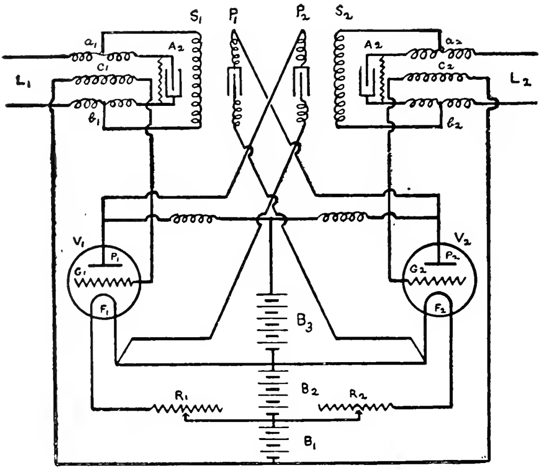


Fig. 23.—Arrangement of Circuits using a Pair of Thermionic Repeaters for Duplex Telephonic Working in both Directions.

laid over again afresh, it is perfectly certain a very great economy in copper could be made by a widespread use of the thermionic valve as a repeater and relay.

It repeats so perfectly that we may certainly say it has completely outclassed all previously invented forms of microphonic relay. The longest telephone line in the world is the direct double line from New York to San Francisco, a distance of 3400 miles. In this line there are three places in which thermionic repeaters are inserted, and by means of which telephonic speech is rendered possible over that vast distance. Without the repeaters the copper invested in the line would have had to be largely increased to obtain equal speech efficiency.

With these brief explanations I must close my story. The development of the thermionic valve has been quite one of the most remarkable things in recent electrical technology.

It was my good fortune to make the first application of thermionic facts to wireless telegraphy in 1904, but the subsequent immense development has been due to the work of many able scientific investigators who were immediately attracted to the new field of research I had opened by its utility and interest.

Without any exaggeration the thermionic valve in its two- and three-electrode forms may be said to be the foundation stones of modern wireless telegraphy and telephony. An enormous amount of valuable research work has been carried out on the action of the instrument in recent years. Thermionic valves have been used literally by millions during the war, but the experience thus gained is being carried forward into time of peace, and has given us new and wonderful additions to our resources for transmitting intelligence to distances quite impossible by any other means.

[J. A. F.]

WEEKLY EVENING MEETING,

Friday, May 28, 1920.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

W. LAWRENCE BRAGG, M.A., Langworthy Professor of Physics, Manchester University.

Crystal Structure.

1. THE examination of the structure of crystals by means of X-rays has made it possible to discover the arrangement of the atoms in a number of the simpler crystal forms. We owe to Laue the original experiments, first published in June, 1912, which placed this power in our hands. In seeking for some means of diffracting X-rays and thus investigating their nature, he was led to use a crystal as a diffraction grating for the rays, the regular arrangement of the atoms in the crystal structure performing the same function as the lines ruled on a grating. The success of his experiment has resulted in investigations which have vastly increased our knowledge of X-rays, of crystal structure, and of the structure of the atom itself.

The problem of crystal structure, which forms the subject of this Discourse, has been attacked in various ways. In his original work Laue obtained diffraction patterns by passing a fine beam of X-rays through a thin plate of crystal, and allowing the diffracted beams to fall on a photographic plate which recorded their geometrical distribution. Though this clearly showed that the X-rays consisted of electromagnetic waves of very short wave-length, diffracted by the atoms of the crystal, the complexity of the resulting pattern on the photographic plate made it difficult to draw conclusions as to the arrangement of the diffracting atoms. A simpler method of attack was realized in the X-ray Spectrometer devised in 1913 by Sir W. H. Bragg. In the course of experiments with which the author was associated, the structure of crystals such as diamond, sodium chloride, zincblende, fluor spar, and the carbonates of the divalent metals were fully worked out, the arrangement of the atoms being determined.

Large crystals are necessary for these methods of analysis, and in order to examine crystalline substances which could not be obtained except as a mass of minute crystals, Debye employed another experimental arrangement which he published in December, 1915. Instead of using a single crystal, Debye passed the X-rays through a mass of finely-powdered material, consisting of crystalline fragments oriented in

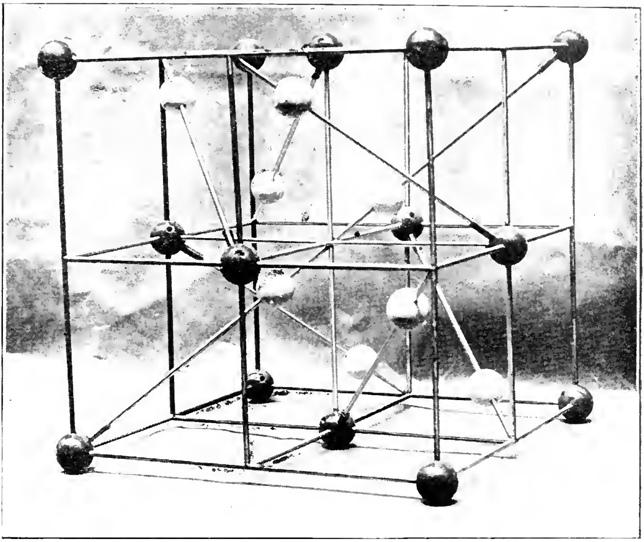


FIG. *a*.—Structure of Iron pyrites, FeS_2.

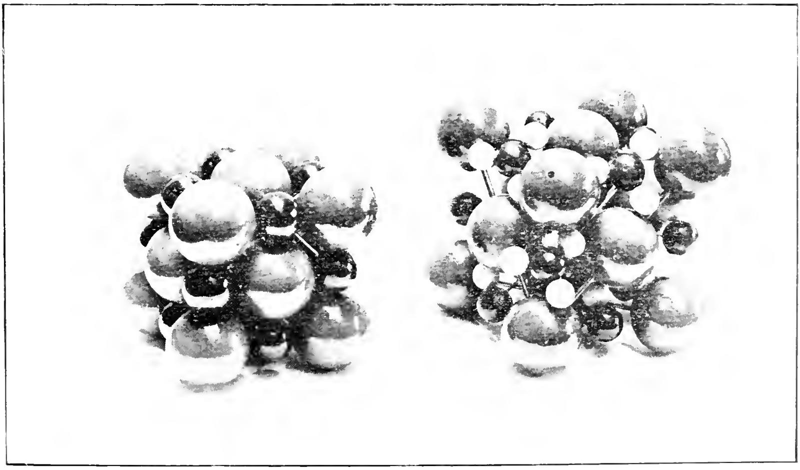
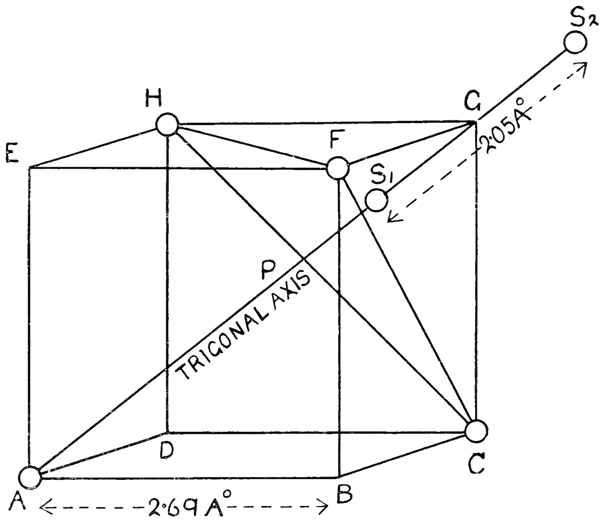


FIG. *b*.—Potassium chloride, KCl; Calcium carbonate, $CaCO_3$.

all directions, the diffraction of the X-rays resulting in the appearance of a set of "halo" rings on a suitably placed photographic plate. He analysed graphite, and showed that so-called "amorphous" carbon consists in reality of minute graphite crystals. The arrangement of the atoms in silicon, tungsten, tin, gold, aluminium and other elements has been discovered by Debye's method. Sherrer has found that colloidal gold and silver consist of minute crystals, the dimensions of which are so small that they are only four or five atoms deep in any direction, and which yet retain exactly the same crystal structure as massive gold and silver. The same method was arrived at inde-



UNIT OF IRON PYRITES STRUCTURE

Fig. 1.

pendently by Hull, and he has extended it to a number of interesting crystals. We now know the atomic arrangement for a very large proportion of the crystalline elements and simple compounds.

2. The essential principle of these methods of crystal analysis may be compared to that involved when a diffraction grating is calibrated by means of monochromatic light, the wave-length of which is known accurately. By finding the angle at which the light is diffracted by the grating, the distance apart of the lines ruled on the grating can be calculated. The planes on which the atoms of a crystal are arranged correspond to the lines of a diffraction grating. A beam

of X-rays of known wave-length falls on the crystal, and it is found that when the beam makes a certain glancing angle with the planes of the structure it is strongly diffracted. By measuring this angle it is possible to calculate the distance between the planes of the crystal structure, just as the distance between the lines of the grating is obtained by employing monochromatic light. The distance between the planes in all directions is thus measured up, and leads to the fixing of the atoms in the crystal structure at their intersections. In this way the crystalline arrangements of some twenty or thirty elements, of compounds such as NaCl, MgO, ZnS, the carbonates, the

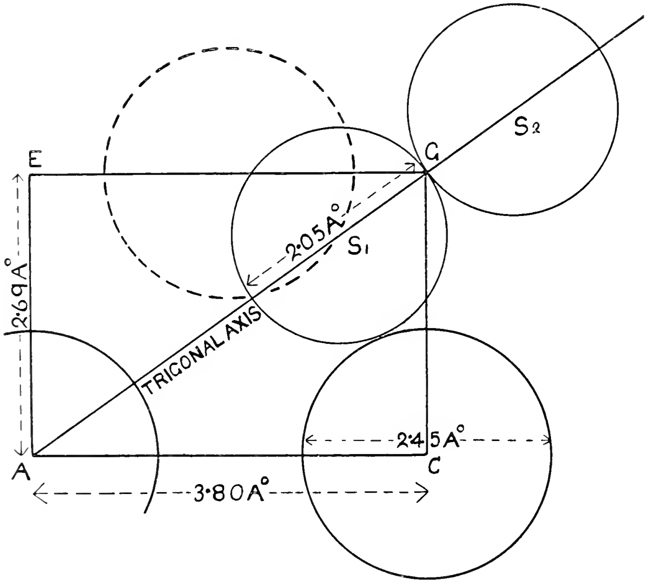


FIG. 2.

spinel group of minerals, the alums, the oxides isomorphous with ruby, pyrites, fluor, galena and many others have been analysed.

3. In many simple crystalline substances the atoms are so arranged that their exact positions are determined by the symmetry of the crystal. In the diamond, for example, each carbon atom is at the centre of four other carbon atoms. In the cubic crystal of potassium chloride, the atoms are arranged so that potassium and chlorine atoms alternate at the corners of the cubes in the structure. Every potassium atom is surrounded symmetrically by six chlorine atoms, every chlorine atom by six potassium atoms. The atoms cannot be displaced

from these positions without destroying the symmetry of the structure, and their exact positions are therefore defined. Such structures are illustrated by the models of potassium chloride and zincblende in Plates I. and II.

In contrast to this, the positions of the sulphur atoms in the crystal of iron pyrites, FeS_2, are defined by symmetry alone. Plate I., fig. *a*, and Figs. 1 and 2 illustrate this structure. The sulphur atoms lie on certain axes of three-fold symmetry, illustrated by the model, and every atom occupies the same relative position along the appropriate axis, but this position must be determined by quantitative investigations of the diffraction of the rays. The ratio of the parts into which the cube axis is divided by the centre of the sulphur atom may be taken as the parameter fixing its position, and this parameter may have any value. In the case of the ruby, Al_2O_3, two parameters are necessary to define the crystal structure; in quartz, SiO_2, four parameters must be determined. The complexity of the crystal structure, and the difficulty of analysing it, increase greatly with the number of these parameters, and it is this which has limited to the simpler forms the types of crystal so far worked out.

4. In trying to find some method of simplifying the analysis of these complex structures, the author has been led to a manner of regarding the crystalline structure which is similar to the well-known theory proposed in 1906 by Barlow and Pope. Barlow and Pope pictured the atoms of a crystal as an assemblage of spheres, packed together tightly, the volume of the space in the crystal structure occupied by the sphere representing any atom being proportional to the valency of the atom. We now know the atomic arrangement of a number of crystals, and we know that the disposition of the atoms predicted by the "Valency Volume" theory, though in some cases it has been found to hold, is in general different to that which the X-rays have enabled us to discover. Nevertheless, Barlow and Pope's models of crystal structures may be modified so as to apply to crystals by substituting, for the valency volume law, one which assigns to the sphere representing any atom a constant size characteristic of that atom.

5. This may be illustrated by the iron pyrites structure already referred to. In this structure the iron atoms are situated on a face-centred cubic lattice. If the unit cube of this lattice be subdivided into eight cubes of half the linear dimensions, each of these latter will have an iron atom situated at four of its eight corners. Fig. 1 represents such a unit of the structure, the iron atoms being at the corners of A, C, H and F. One diagonal of each cube, the diagonal AG in the figure, is an axis of threefold symmetry, and the sulphur atom lies at some position along this axis. Since each corner of the cube is a centre of symmetry, there will correspond to the sulphur atom centred at S_1 a similar atom at S_2, where $S_1G = S_2G$. A pair of sulphur atoms are thus associated with each cube corner, since one

threefold axis passes through every corner, and the necessary proportion of two sulphur atoms to one iron atom is realized.

On the conception of the crystal as a set of spheres packed together, it will be seen that there are two possible positions for the sulphur atom. It may lie at the centre of the cube, where it is surrounded by the four iron atoms at A, C, H, and F, an arrangement which exists in the structure of fluor spar, CaF_2. Alternatively, it may move along the axis to a position G_1 on the other side of the plane HFC, where it will be packed between the three iron atoms at H, C and F, and the corresponding sulphur atom at S_2.

If the size of the sphere representing the iron atom is known, geometrical considerations fix the centre of the sulphur atom touching the iron atoms and the other sulphur atom. As a first approximation to the size of the iron sphere, the distance between atomic centres in metallic iron, first analysed by Hull, may be taken. This distance is $2 \cdot 47 \times 10^{-8}$ cm., or $2 \cdot 47$ Ångstrom Units. Using this value, the ratio S_1G/AG is found to be equal to $0 \cdot 22$. The author originally deduced the value $0 \cdot 20$ for this parameter, and a more exact determination by Ewald gave it as $0 \cdot 226$, so it will be seen that the conception of the atoms as a set of spheres packed together leads to a determination of the parameter very near the true one. Further, the diameter of the sphere representing the sulphur atom follows to be $2 \cdot 05$ Å. Fig. 2 will illustrate the manner in which this result is arrived at.

6. The structure of zinc sulphide, and therefore the distance between the zinc and sulphur atomic centres, is known; it is illustrated by Plate II., fig. *a*. Taking the diameter of the sulphur sphere to be $2 \cdot 05$ Å, that of the zinc sphere touching it is found to be $2 \cdot 65$ Å. From the zinc oxide structure the dimensions of the oxygen sphere can be calculated; its diameter is found to be $1 \cdot 30$ Å. This difference of $0 \cdot 75$ Å between the diameters of the oxygen and sulphur spheres is also found to exist in a number of other crystals.

The rhombohedral carbonates typified by calcite, $CaCO_3$, provide a check on these figures. We have seen that the oxygen sphere has a diameter of $1 \cdot 30$ Å. In the diamond the distance between the carbon centres is $1 \cdot 54$ Å, and this may be taken to be the diameter of the carbon sphere. Every carbon atom in the carbonates is closely surrounded by three oxygen atoms, and we should therefore expect the distance between carbon and oxygen centres to be $\frac{1}{2}(1 \cdot 54 + 1 \cdot 30)$ Å, or $1 \cdot 42$ Å. As a matter of fact the X-ray measurements show it to be $1 \cdot 47$ Å, which is in fair agreement with the calculated value. Further, in zinc carbonate the zinc atom is surrounded by six oxygen atoms, the distance between the centres being $1 \cdot 99$ Å. Comparing this with the distance $1 \cdot 97$ Å between zinc and oxygen

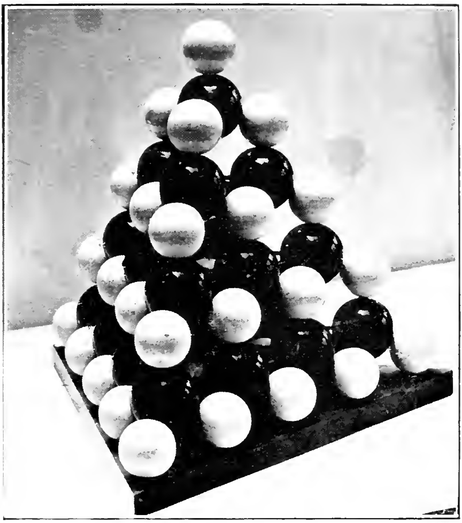


FIG. *a*.—Zincblende, ZnS.

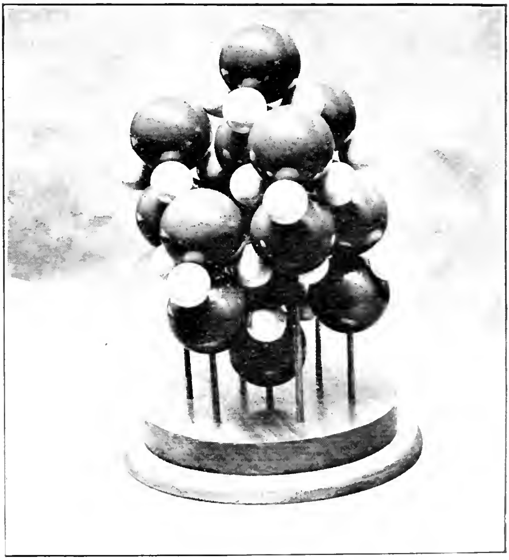


FIG. *b*.—Alumina, Al_2O.

centres in zinc oxide, we see that each of the atoms appears to occupy a space of the same dimensions in all these crystals, sulphur in pyrites and zinc sulphide, zinc in zinc sulphide, oxide, and carbonate, oxygen in zinc oxide and carbonate, carbon in the carbonate and in diamond.

7. This will illustrate the manner in which the atomic diameters shown in the diagram (Fig. 3) have been calculated. The results may be summarized by saying that the distance between two neighbouring atoms in a crystal structure is equal to the sum of two constants characteristic of the atoms. We can therefore picture the atoms as a set of spheres packed together so that they are in contact, by taking these constants to be the radii of the spheres, and the models which illustrate this lecture have been built up in this way.

In the figure the elements are arranged in the order of their atomic numbers, and the ordinates represent the diameters of the corresponding spheres, measured in Ångstrom Units. A comparison of the distances found between the atoms in these structures which have so far been determined, with the distance calculated by adding the "atomic radii," shows that the average discrepancy between the two is 0·06 Å, or between two and three per cent.

It will be seen that the atomic diameters lie on a series of regular curves which show very strongly the periodic arrangement of the elements. The monovalent electropositive elements have the greatest diameters, the divalent metals the next greatest, and passing along each period the diameter diminishes steadily until it approaches a lower limit for the electronegative elements at the end of the period. In the case of two of the elements, chromium and manganese, it is necessary to suppose that the atom has a smaller diameter when functioning as an electronegative element than as an electropositive one.

The curve may be compared to Lothar Meyer's curve of atomic volumes, but in the case considered above the volume is that occupied by the atom in all the compounds into which it enters, so that the curve has a wider application. The molecular volume of a compound often differs very greatly from the volume obtained by adding the atomic volumes of its constituents, but if the crystalline structure is taken into account, this generalization shows that the space occupied by each atom is approximately constant.

In some cases the distances between atoms do not agree with those calculated from the diameters; for example, this is the case for many of the elements. In spite of this, the analysis of a complicated crystal is greatly helped by this way of regarding the atomic arrangement. When marshalling the atoms together in trying to find some arrangements which account for the diffraction effects, the necessity of allowing each atom a certain space in the structure limits the variable parameters to a small range and so simplifies the analysis.

8. The physical interpretation of these empirical relations must be sought for in the structure of the atom, and in fact that theory of

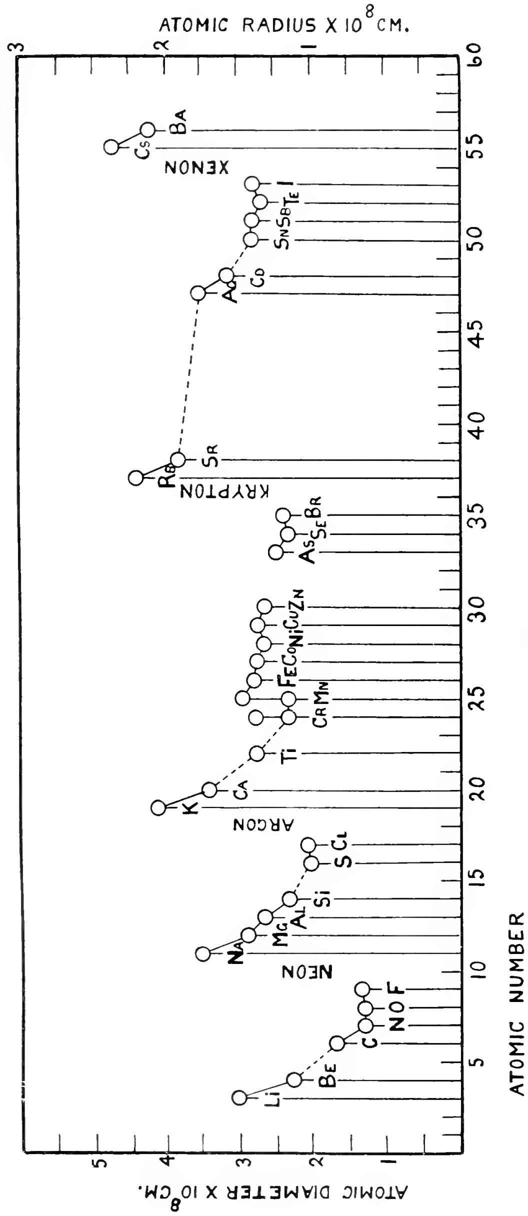

ATOMIC RADIUS X 10⁸ CM.
ATOMIC DIAMETER X 10⁸ CM.
ATOMIC NUMBER
Li Be C N O F
NEON
Na Mg Al Si S Cl
ARGON
K Ca Ti Cr Mn Fe Co Ni Cu Zn
As Se Br
KRYPTON
Rb Sr
Ag Cd Sn Sb Te I
XENON
Cs Ba

Fig. 3.

atomic structure, which has been developed by the work of Kossel, Lewis, Born, Landé and many others, and which has been so strikingly summarized and extended recently by Langmuir, affords a ready explanation of the relationships. An essential feature of this theory is the supposition that the electrons, which surround the positive atomic nucleus, are fixed at, or oscillate about, certain definite positions in the atomic structure. This may be contrasted with the type of atomic structure represented by the Bohr atom, when the electronic orbits are supposed to enclose the atomic nucleus. In the "fixed electron" atom the electrons are placed in certain definite positions in a series of concentric spherical shells surrounding the nucleus. The commencement of each period of the periodic table marks the commencement of a new shell, and the inert gases which separate the periods are the atomic systems in which the outer shell has in each case its full complement of electrons.

The arrangement of electrons in an inert gas is a very stable one, corresponding to the chemical inactivity of the elements. The chemical properties of the other elements are due to the tendency of the system to revert to a more stable arrangement, such as that of one of the inert gases. Argon, for example, with atomic number 18 has a stable system of 18 electrons surrounding the nucleus. Potassium has an atomic number 19, and the nucleus is surrounded by 19 electrons in a neutral atom. There is, therefore, a tendency for the potassium atom to lose an electron and revert to the stable argon arrangement of 18 electrons, so that potassium behaves as a monovalent electropositive element. Chlorine, on the other hand, with atomic number 17, tends to gain an electron, and is a monovalent electronegative element. The atoms which have thus lost or gained an electron are positively and negatively charged: they are the kations and anions of potassium and chlorine.

The atomic numbers of the inert gases tell us the number of electrons in the outer shell which each atom must have for complete stability. The atomic numbers of helium, neon, argon, krypton, xenon and niton are 2, 10, 18, 36, 54 and 86, so that the number of electrons which must be added to complete each successive shell must be 2, 8, 8, 18, 18 and 32.

9. On this theory, two different types of chemical combination may be distinguished. The first is that represented by such a compound as potassium chloride. Here the potassium atom has lost an electron in reverting to the argon arrangement of eighteen electrons, and the chlorine atom has gained one. The oppositely charged ions are held together by the electrostatic attraction of their resultant charges. In the crystal of potassium chloride the positive potassium ion tends to surround itself with as many negative ions as possible. This is realized in the crystal structure (Plate I. fig. *b*), where each ion is surrounded by six of the opposite sign. The fact that the "molecule" of KCl has no apparent existence in the

crystal structure receives a natural explanation on this theory. The valency of the ion of either sign is due to an electrostatic attraction and can be subdivided to any extent.

The other type of chemical compound is that of two electro-negative elements. Each element in this case has a smaller number of electrons than is necessary for complete stability. In order that the empty spaces in the outer shell may be completely filled, the atoms share electrons. the valency bonds corresponding to a pair of electrons held in common by both atoms. It is in this way of regarding the combination of electronegative elements that the Langmuir theory finds one of its strongest supports; the complicated valencies of elements such as nitrogen and phosphorus are readily explained by a consideration of the ways in which these atoms can fill up their outer shells by holding electrons in common with other atoms.

10. It has been seen that the potassium chloride crystal consists of alternate ions. The structure of calcite (Plate I, fig. *b*) presents the same alternation of ions, only in this case one of the ions. the CO_3 group, is complex. The calcium atom has lost two electrons in reverting to the argon arrangement of electrons, and it is therefore a doubly charged positive ion. The CO_3 group has absorbed into its system two additional electrons, and the four atoms in the group are surrounded by the total complement of electrons for stability, some of the electrons being held in common. In this crystal both types of chemical combination are illustrated: the calcium and CO_3 ions are held together by their charges; the carbon and oxygen atoms are bound together into the CO_3 group by holding electrons in common. The crystal is therefore divisible into units, each unit having a continuous outer electron shell. One unit is the calcium nucleus surrounded by its proper electron cloud, the other is the CO_3 group again surrounded by its electron cloud. Some repulsive force must be supposed to exist between the outer electrons which keeps the ions apart, opposing their electrostatic attractions for each other. The arrangement of the ions in potassium chloride and calcite is the same, except that in the latter case the substitution of the complex CO_3 ion for the Cl ion distorts the cube into a rhombohedron (cp. figure).

11. The empirical relations summarized by Fig. 3 can readily be interpreted on this theory. In calcium carbonate. for example. the proximity of the carbon and oxygen centres leads to a small diameter being assigned to the spheres representing these atoms. while the comparative isolation of the calcium atoms leads to a large diameter being taken for the corresponding sphere. We now see that the carbon and oxygen atoms are close together because they share electrons; the calcium atom is isolated because it has no electrons in common with the other atoms. The large diameter assigned to the electropositive elements does not therefore indicate that the outer electrons are any further from the nucleus than they are in the

electronegative elements of the same period: it expresses the fact that the electropositive atom never shares electrons with the neighbouring atoms, and is therefore always found to be at a distance from the other atomic centres as if it occupied a large domain in the crystal structure.

12. In each period the divalent ion is assigned a smaller diameter than the preceding monovalent ion. Sodium fluoride and magnesium oxide, for example, have exactly the same atomic arrangement, that of potassium chloride. The dimensions of the magnesium oxide structure are, however, smaller than those of sodium fluoride: the side of the unit cube is 4·22 Å, as compared with 4·78 Å. Since other data assign practically identical dimensions to oxygen and fluorine, the difference in the dimensions is accounted for by taking a smaller diameter for magnesium than for sodium. A comparison of the identical stuctures, magnesium carbonate and sodium nitrate, leads to the same result. We may ascribe this closer grouping of the atoms in the magnesium oxide or carbonate to the fact that the charges on the ions are double those in sodium fluoride or nitrate, so that the forces of attraction are more than four times as great. For the same reason the trivalent elements appear to occupy a still smaller domain in the crystal structure.

13. At the end of each period the diameters approach a lower limit. Since the electronegative elements are holding electrons in common, an estimate of the size of the outer electron shell can be formed from the distances which separate the atoms. The limiting values for the periods are, approximately:—

Neon	1·30 Å
Argon	2·05 Å
Krypton	2·35 Å
Xenon	2·70 Å

The diameters of the outer electron shells would appear to be equal to, or slightly less than, these values.

14. We can now review the types of crystalline structure. In what follows the various points have either been made by Langmuir or are direct consequences of the atomic theory which has been described above.

The crystals of a salt have been discussed above. Crystals of sodium chloride, or calcium carbonate, consist of ions of opposite signs. The ions are surrounded each by its own electron cloud; they are held together by their electric charges, and kept apart by repulsive forces which must be supposed to exist between the outer electron shells. Diamond represents yet another type of crystal. The carbon atoms have each four electrons in the outer shell, and the complement of eight is attained by each carbon atom holding in common a pair of electrons with the four other atoms surrounding it symmetrically. The whole crystal is one molecule, all the atoms

being linked up by electron sharing, a view which is supported by the density and hardness of the diamond. A crystal such as quartz probably has the same type of structure. The structure of carborundum resembles very much that of diamond, and here again every atom shares electrons with all its neighbours.

The crystal of an electropositive element, such as lithium or potassium, has a different structure. Each atomic nucleus has a stable arrangement of electrons surrounding it, corresponding to one of the inert gases, and in addition one or more electrons must be associated with each atom according to its valency, as the whole mass of the metal is electrically neutral. The crystal may be regarded as composed of ions and electrons, the electrons having no definite place in the crystalline structure. If an electromotive force is applied to the metal they are driven through it; in other words, the metal is a conductor of electricity.

Still another type of association is that represented by water of crystallization, where some residual forces of attraction must be supposed to hold the electrically neutral water molecules in the crystalline structure.

15. Crystals where all the atoms hold electrons in common are characterized by great hardness; large forces are necessary to break the atoms apart. The typical salt, on the other hand, is soft, as in parting the atoms the only forces which have to be overcome are the electrostatic attractions between the atoms. An exception occurs when the ions have a double or treble charge and are close together, as in magnetite, Fe_3O_4, and ruby, Al_2O_3, for then the forces must be supposed to be so great as to give the crystalline structure considerable hardness.

In the crystal of a metal the ions, held together by electrons which probably have no fixed positions in the structure, are still freer to move past each other and give the metal its characteristic malleable and ductile properties.

Since the first two classes of crystals have no free electrons, they are non-conductors of electricity. This is the case for crystals of the electronegative elements where the atoms hold electrons in common. It is the last class of crystals, those of the electropositive elements, which are conductors.

16. In studying crystal structure we are studying the arrangement of the atoms in the solid state, for all true solids are crystalline in nature. It is the extremely short wave-length of the X-rays which makes this feasible. The limit to the minuteness of detail which it is possible to see under the microscope is fixed by the wave-length of the light used to illuminate the object, for it is impossible to distinguish two objects which are closer together than that wave-length. In the case of X-rays the wave-length is ten or twenty times smaller than the distances between the atoms, so that by illuminating the crystal with X-rays the atomic arrangement may be

analysed. It may be possible to push the analysis still further, and find in a direct manner the arrangement of the electrons around the nucleus. Experiments on these lines have already been carried out; for example, Debye's experiments on lithium fluoride.

17. I have tried to indicate the contribution which the study of crystals has made towards the solution of the problem of atomic structure, and the lines on which it may be expected to afford a still greater insight into that problem in the future. In considering the broadest aspects of the question it has been impossible to avoid generalizations to which many exceptions occur. I hope, however, to have shown that crystal structure has a wider significance than may perhaps be obvious at first sight. In a crystal the atoms and atomic groupings are ranged in perfect geometrical regularity, and it is by observing their concerted effects on the X-rays that we can investigate the intimate structure of solid bodies.

[W. L. B.]

WEEKLY EVENING MEETING,

Friday, June 4, 1920.

Sir James Crichton-Browne, J.P. M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

Colonel Sir Ronald Ross, K.C.B. K.C.M.G. F.R.S.

Science and Poetry.

In the forefront of my dissertation to-night, let me thank those who so ably conduct the affairs of the Royal Institution for the honour they have done me—and the kindness they have done me—by inviting me to address you upon a theme which has always been much more congenial to me than is another with which you will perhaps more readily connect me. Twenty years ago I had the privilege of telling you about the great good fortune which had then just befallen Science when she discovered how malaria, that tyrant of the whole tropical world, maintains his empire over us; and years later I informed you, with some anger (as I remember), of the difficulties which we experienced in persuading our comfortable countrymen to employ that knowledge for the defeat of the enemy. I am now asked to address you upon a very different and perhaps a still mightier theme, that of Science and Poetry; and you may query at the outset what connection there is between all these matters, and what right have I, individually, to speak upon them.

In answer I would point out that this Institution, which for a hundred and twenty years has kept burning the Vestal Fire of the human intellect in this country—a fire which often smoulders so dimly in the hearts of our people—that this Institution has always upheld what I take to be the first charter of the spirit of man, the right to soar (or to sink) in whatsoever direction it pleases. The object of the Institution has always been to promote the study both of science and of literature. It does not concede to the fashion of the day that would fasten the blight of Indian caste upon us; that would make us either literary men or scientific men, either business men or professional men, either tinkers or tailors. Not only have its members heard in this hall regarding every advance in science almost as soon as made, but they have also listened to Coleridge speaking on poetry in 1807 and 1808, and to Campbell on the same theme from

1812 to 1815. And still less have its distinguished audiences consisted only of the acolytes of science, for Tennyson and Browning attended lectures here ; and so I think has every man of eminence, at some time or other. And I will say this, as a first approximation to my subject of Science and Poetry, that it is not only the right but the duty of the spirit to explore every direction, if only to learn the limits of things. In intellectual affairs, the cobbler shall not always stick to his own last, lest he become only a mere journeyman.

It is true that much specialization is needed in order to reach technical perfection, both in art and in science ; but technical perfection should be only the flower of a tree the roots and branches of which spread on every side in the air and soil of experience. There is a limit to the merits of specialization : nor should we agree with the dictum, which I have seen stated, that every great poet must be a professional poet, that is, a literary man, and that every man of science should concern himself only with test-tubes and microscopes. Still more do I abhor the superstition that every branch of every art and science should be further sub-divided. This is not the teaching of history, so far as we possess the histories of the greatest men. As everyone knows, Michael Angelo was sculptor, painter, architect, and poet. Leonardo da Vinci was painter, mathematician, mechanician, military engineer, and father of many inventions. Descartes created not only analytical geometry. When Peter Paul Rubens was ambassador in England, an English courtier called on him and found him seated at his easel. "So His Excellency the ambassador plays at being a painter," exclaimed the courtier. "No," replied Rubens, "His Excellency the painter plays at being an ambassador." In the days of Voltaire, philosophy contained all the sciences and discussed all the arts. Goethe commenced, not only a literature, but the theory of evolution. I wonder in what witch's-cauldron of folly the absurdity was brewed that poetry and science are enemies. Shelley tasted several sciences, and, when he was in Italy, proposed to make a careful study of mathematics. The poems of Coleridge were indeed flowers that peeped out from among the rocks of his philosophy. Keats was a medical student, and I am convinced would have shown, had he lived, how poetry may descend from the shrine of science ; and he has already nearly summed up my theme of to-day in his apothegm, "Beauty is truth, truth beauty." Hugo, Tennyson, Browning, Swinburne, and especially Arnold, all followed science more or less closely. Indeed, it is never among the greater poets that we notice any antipathy to science, in its broadest or its narrowest sense. Perhaps sometimes poets may be somewhat chilled by the cold pure water of science, but that is only when they were (as poets are apt to be) tempted by the foaming but less wholesome drinks of, let us say, nescience. It is the lower type of what may be called literary poetry which, like much of our purely literary philosophy, endeavours to attack science.

And conversely, as poets have been interested in science, so have men of science often written verse, and sometimes very good verse. The case of Francis Darwin is known to all: and many papers have been published on the medical poets. Edward Jenner, the great discoverer of the means of preventing one of the most horrible scourges of humanity, small-pox, was one of them. Sir Humphry Davy, the brilliant chemist, the discoverer of a whole group of elements, the inventor of the safety-lamp, and one of the brightest stars of the Royal Institution, was such a fine poet that Coleridge said of him, "If Davy had not been the first Chemist, he would have been the first Poet of his age." Let me quote the following lines by him, headed Ravenna, March 1, 1827, as an example of his musical verse:—

Life we term a spark, a fire, a flame;
And then we call that fire, that flame, immortal,
Although the nature of all fiery things
Belonging to the earth is perishable.
The lightning, in its fierceness and its power,
Is of an instant only!
The meteor's blaze lightening the visible scene
As transient is!
And vainly should we search where these had been.
The solar light, when the bright orb has sunk,
Dwells not within known space;
And that which kindleth the whole frame of nature
Has no abiding place, although its source
Is everlasting.

We read of Davy that when he was at school he developed a taste for telling stories, poetry, angling, and experimental science; that he was apprenticed to a surgeon, and entered upon an encyclopædic course of study. But I fancy that many scientific men, and indeed many others, have had a similar history; and, though I dare not mention names, I suspect that even some of our most distinguished professors to-day have been guilty at least of a five-act blank-verse drama in youth; while many poets have certainly secretly dabbled in chemistry and electricity. I admit for myself (though I dare not call myself either) that at the age of twelve I set forth to write a zoology which should contain all known species of animals (some millions in number, as it happens), with illustrations, and with a poem attached to many of the descriptions—as of the roaring of the lion at midnight, and of the snapping up of children on the banks of the Hugli by appalling crocodiles!—and indeed some of these poems have been actually printed, much changed, in my Fables in 1907. This kind of thing, the fury of youth, is common to all of us; but why did Davy enter upon an encyclopædic course of study? What is really the psychology of such an ambition, which appears so dull to so many people? It is the basis of the psychology both of poetry and

of science; and I suppose that everyone here, except perhaps the very wisest of us, whom I will not name, have suffered from it. Psychology, like charity, begins at home: and you may ask what was my own encyclopædic course of study, and why on earth I undertook it. Before I was twenty-five, I had not only progressed in my zoology, but had written two of the said five-act dramas besides numerous lyrics; had modelled the figure of Prometheus chained to the rock; had commenced an oil-painting of the fall of the rebellious angels (in the manner of Michael Angelo): had composed a number of sonatas and songs; had passed through my medical curriculum; had seen something of life and sport, of land and sea; and, finally, carried away by reading a book on astronomy, had laboured right through the mathematical works of the ponderous Isaac Todhunter, F.R.S., and had created a theory of matter which has never been proved only because I could not solve the mathematical problem involved! This seems a large programme, but it is not so; it is, with variations, the common programme of youth, and I suppose that many of you have had similar ones. But why do we take so much trouble? Does anyone really believe that young men do this kind of thing in the pursuit of knowledge—what, in order to keep stuffed facts in glass cases in the museum of the mind? Or do they do it out of vanity, as shallow psychologists (including the great Lord Verulam) declare? Why, as you are aware, if others knew of our ambitions we should have only been ridiculed for our pains! No, it is the force which makes every young man climb the first mountain he sees, in order to reach the top and look round. It is the force which makes the explorer, the big game hunter, the arctic voyager, the inventor, the philosopher, the true statesman. It is the force which has been implanted in us by the evolution of ages, in order to perfect the human race. It is the force which creates both poetry and science. It is the force which leads us step by step out of the jungles, ever towards the final godhood of man. It is the prime and the sublime æsthetic element of the soul—the sense of beauty, the desire for perfection.

This is a matter worth your profound investigation, for both science and poetry are to-day being attacked by a generation of very poor criticism; and I was glad to see that Mr. Alfred Noyes, in this hall on January 24, and Sir Reginald Blomfield, R.A., before the British Academy on May 5, undertook something of the defence of real art. Do you really imagine that science is concerned only with the discovery of petty utilities; art with the discovery of new tricks of technique; and literature with mean books written by, for, and about mean people? There are even certain schools which dare to maintain that both science and art have nothing to do with ethics, with teaching, with the advancement of the race. If this were the case I for one would have had nothing to do with either of them. I say, not art for art's sake, nor science for the sake of science, but

both for humanity. In every great work of art which I have ever examined I see the didactic intention written large all over it. Art is science teaching us, not by means of saws and syllogisms, but by means of wise instances and great figures set within crystals of perfect and immortal beauty. It is this very didactic intention which divides great art from little art. Take, for example, what are perhaps the most enduring and universal of all forms of art, proverbs and maxims, the fairy-tales, folk-lore and mythologies. The latter may perhaps contain personifications of the dawn, the totem and tabu; but they contain much more of wise instruction; and my own feeling is that they are the relics of great poems invented long before the days of writing, in the vast dark ages of mankind, by the prophets and thinkers who lived then and who had no other means of communicating their wisdom. The mythology of Greece was created by Homers before Homer, and is a perpetual instruction which permeates us all even to-day. The court of Zeus, the labours of Heracles, the despair of Orpheus, are pictures of universal fact; and the punishment of Prometheus for giving fire from heaven to men, that is, the punishment of genius, is a story of which many actual instances may be mentioned. Then take the world's greatest literature—let us say, Homer, Dante, Cervantes, Shakespeare. As I have argued elsewhere, these great men of science were, each in his own epoch and country, the first to commence the exposition of a branch of natural knowledge which, though it is of prime importance for all of us, has not yet even received a name. The *subscience* of this department of knowledge aims at collecting, classifying and cataloguing the infinite varieties of character and circumstance found in human life; the *theory* of the science attempts to extract from the facts an explanation of human action; and the great final *synthesis* endeavours to give us a logical rule of virtue and conduct based upon the previous findings—just, as for instance, chemistry tells us how to make sulphuric acid out of certain elements. Now the only manner in which such a science can be taught to men is by way of stories which, though they may not actually have occurred as described, are really occurring over and over again—somewhat as Euclid's book was the first to crystallise geometry in sets of propositions with figures which are never actually found in nature. The constructions of the men of science mentioned above are similarly idealised, partly for brevity and partly for fixing the attention of the public. With them, as with Euclid, this necessity demands crystallisation in the highest æsthetic sense, the perfection of form—a crystallisation which is known by the name of art. Fundamentally, however, all these works are works of science. The art lies only in their presentment.

I say that it is the philosophic intention which distinguishes great from little art. Consider this intention in the books of Homer. The

"Iliad" is a picture of Force, set forth so as to show every facet of the subject, the triumph of Force and the evil of it. The work begins by showing the great failing of Force, unreasonable wrath—the whole world being made to suffer; and then Force returns to its duties in consequence of a more noble but equally unreasonable grief. This is a primordial lesson for humanity, which exists in fact under the influence and government of Force. On the other hand, the "Odyssey" is the great allegory of Wisdom; and in order to form the picture fully, Homer plunged Ulysses in difficulty after difficulty that by his wisdom he might finally emerge. This again was a prime lesson for humanity.

The Greek tragedies and comedies give us lesson after lesson of the same kind; and so do the Latin poets. In quite another wave of civilisation Dante wrote another parable, the function of which was to impress something which the Greeks had not fully impressed, the sense of a more matured moral obligation. The masterpiece of Cervantes states in immortal and unforgettable figures two extremes of the mind, idealism and realism—qualities constantly in conflict, but which must nevertheless always work together. Similar design exists in every one of the plays of Shakespeare. In "Macbeth" ambition; in "Othello" jealousy; in "Romeo and Juliet" noble love; in "Lear" ingratitude; in "Julius Cæsar" political jealousy; in "Antony and Cleopatra" political profligacy; and in "Coriolanus" pride—to use only brief appellations. His "Hamlet" and "Timon" may almost be looked upon as medical text-books—Hamlet of excessive introspection, and Timon of some form of enterosepsis. In his last play, "The Tempest," he figures, I think, himself as Prospero, the wise and gentle magician, giver of the wonderful and beautiful Miranda to the world, but exiled by baser people; and in Caliban (who hated him but followed a drunkard and a jester), the monstrous stupidity and indifference of the world. Lastly, in the Greatest Book of all, the most sublime drama of the Gospel, we have the final instance of what human conduct should be, as well as the final tragedy of virtue and self-sacrifice—to warn us lest we think that virtue may have any other reward but itself. In the great cathedral which is the spirit of man, each of these books is a chapel by itself, beautifully adorned, lit by light from Heaven, and existing for ever in the sacred silence of real holiness.

I have no space to deal with other masterpieces concerned with scientific or philosophical life. The poem of Lucretius is, of course, the scientific poem of the world. Marlow's "Dr. Faustus" is the picture of the scientific mind tempted away from its proper labour by pleasure; and Goethe's "Faust" of the scientific mind experimenting in evil. I think that Matthew Arnold's "Empedocles" is perhaps the profoundest poem of last century (not excepting "In Memoriam"), with its able picture of the staleness and despair which too often over-

come the much-labouring philosophic mind, and its two wonderful final lyrics. The lines—

> Yea, I take myself to witness
> That I have loved no darkness,
> Sophisticated no truth,
> Nursed no delusion,
> Allow'd no fear,

express the very spirit of true science.

The criticism of the day affects to deride all philosophic and didactic intention, and, in its small way, considers only the artistry of the surface. It is true that all these great works are immortal not only because of their philosophic and didactic intention, but because of their art of presentment. But the latter is only the servant of the former and not its master. You may exclaim against this; you may ask where is the science in the great lyrics, in those most poignant moments of passion or beauty which suddenly appear in dramatic, rhetorical or descriptive literature? Where is the science, for example, in Gray's "Elegy"? I say that those moments are the final summations, the supreme integrations of the spirit. The "Elegy" is the integration of all the sights and sounds of an English summer evening, and of the reflections connected with them, crystallised in a few lines of perfect form and music which remain in the memory for ever. It is art. Yes: but it is not only art. Behind the art there is an almost divine summary of things. So also, for example, in Shelley's "Ode to the West Wind," when he exclaims:

> Be thou me, impetuous one!
> Drive my dead thoughts over the universe
> Like withered leaves to quicken a new birth!

he is conscious of a mighty impulse behind that æolian harp which the spiritual fingers of his genius sounded, and which was his art. We see the same thing in all the other arts. Music, although she has no words, teaches by the direct inspiration of beauty into us; and in the prime, and also the ultimate, sculpture of Greece we find not only the beauty of nature, of the human form, but behind it the summary of the sculptor's mind, his ideal of the godhood of man. In short, the world's masterpieces consist, not of one thing, but of two things commingled together for our perpetual instruction, the spirit of discovery and the spirit of expression, or rather of instillation. Or, I may put it in this way. These forces are to the mind what the great Calculus is to Mathematics: Science, the Differential Calculus which separates, subdivides, and analyses; and Poetry, the Integral Calculus which sums up. Nor is one ever complete without the other: and in my view—which is, I fear, contrary to much of the

literary opinion of the day—the poet should begin by being the man of science, and the man of science by being a poet. And in fact that is what has occurred with many of the greatest men.

I should have liked to analyse the great scientific poems one by one (which has never yet been done), but I have been particularly asked to give in this lecture some of my humble essays—perhaps only to show how easily my theories break down in my own practice! After the names which I have cited, I scarcely dare even to mention that I have ever made any efforts at all in this direction! But I must obey and do my best; and will therefore try to indicate very briefly the gradual development of my own thoughts both in science and in poetry—that is, my ideals.

I should like to begin in a light vein with one of my Fables, written nearly forty years ago when I gave over my boyish pursuit of the arts for the study of mathematics. It is called the "Poet's Retirement." The poet (idealised) on descending the Hill of Youth, meets those three maidens, the Arts, who persuade him to visit their own beautiful domains; but while he is trying to make up his mind which of the Arts he prefers, the Muse of Science enters and carries him off!

The Poet's Retirement.

Down from that blithe Idalian Hill
Where Violets drink of dew their fill,
And wading thro' wet eastern flowers
With wash'd feet Eos and the Hours
Come laughing down, I laughing came.

The Morn had now her threads of flame
Inlaid to Earth's green tapestries,
Gold-inwoven; and to their knees
In chilly baths of thridding rills
At tremble stood luce Daffodils;
When, lo! I marked toward me move
Those Maidens Three whom poets love.

"O whither away, glad Youth," they cried,
"Singing thro' daffodils dost thou stride?"
"Ladies, I wander for a while"—
And here I duck'd and doff'd in style—
"I wander by Bourn, I wander by Byre,
By Cape and Cote and Castle Spire,
Or sometimes stick in puddled Mire;
Or climb the summits of Snow and Fire.
Or where the hoarse moon-madden'd Tides
Drench dripping jags on Mountain sides;
Or twanging strings sound gay reprieve
To smoky Villages at eve,
What time the paddock'd Ass careers
Mirthful, with high-prickt tail and ears,

And slow towards their wattled home
The baaing Sheep do go, I roam.
And I have left behind me there
Hippocrates teaching the air;
And Learning prim; and Venus too
Now whipping Cupid with her shoe."

Then, of those slipper'd Maidens, She
Robed in flush rose-red answer'd me,
Who brightly gazing with mild look
Held still a finger-parted book.
"Come then," she cried, "with me and dwell
In my Valley of Asphodel,
Which is a land of laughing rills
And hung about with dazzling hills,
Where oft the Swain with garter'd legs
Piping for love in music begs,
Nor Thisbe turns her petulant ear.
There large-eyed Plato thou may'st hear
Persuade, or, if not idly awed,
Masters a Master's theme applaud.
And then, if Thunder more invite
Than silver-threaded rain's delight
And sloping seats of knollèd moss,
Come where some thwarted Torrent toss
Thro' cloven Gorges, mad to shake
The shagreen'd Boulders black and break
The gleaming silence of the Lake.
Or, if engross'd with human Fate,
On rangèd boards mark Love and Hate
Egg on to midnight-living crime,
And glaring Horrors of dead time
Creep in behind. Or, restive still,
Unlock'd from Hell soar Heaven's hill
Thro' sun-outstaring Cherubim."

"Not so," cried one, a Virgin slim,
Plumed, wrap'd and robed in the gold-green
Thro' sunset-dazèd woodlands seen;
Who half upon her dinted breast
Apollo sculpt in little press'd.
"Come to my House of all delights,
Whose marble Stairs with merged flights
Are shallow'd in the viewless Lake;
Whose overpeering Turrets take
The peep of Dawn, or flashing turn
To Eve departing golden scorn.
There fairy-fluted Pillars soar
To cloudy Roofs of limnèd lore,
And Walls are window'd with rare scapes
And rich designs; of blazon'd Capes
Pawing the sunset-burnish'd flood;
Of rib-rail'd reaches of Solitude;

Of rounded World and globèd Skies,
And Stars between, and faint Moonrise ;
Of black Tarns set 'mid mountain peaks
And spouting silver-foamèd leaks ;
Of Gods reclined, and Maids who move,
Unlidding lustrous eyes of love ;
Of War ; of Wisdom with a skull.
And in the high aisles Fountains full
Disperse a stream of coolness there
For frosted fern and maidenhair,
And sculptured Beauty holds the way.
So thither go with me to-day."

Then She who all in purple dight,
Brow-star'd with orbèd ruby light,
Lifted from under rich deep locks
Looks wrapt on Heaven, to earthly shocks
Descending, thus replied : " Not these
Flat hapless lands of Towers and Trees
May past the morn your spirit please.
But to some cold Crag, that doth lift
His brow to Heav'n above the drift,
And turns beneath the mistless Stars,
Come. There no dew distilling mars
With felon fog or frozen haze
The many hued Sidereal blaze,
Where Planets pale not age to age,
And moonèd Venus in white rage
Stares down the Dawn. Come ; for that Glow
There solves to unpolluted flow
The crumbling crystals of the Snow ;
And windworn Cataracts wavering plumb
To lightless pine-valleys. Come, O come !
Lest those faint Harmonies be unheard
Which, as from silver and gold strings stir'd
By the light fingers of the Wind,
Run from the poised orbs swiftly spin'd."

She ceased, and with her finger tip
Made sound the lyre upon her hip,
And would have sung ; but I replied,
" To be unchosen is descried ;
And we shall be made mad in Heaven
By need of choice of good things given.
I love all Three so passing well
Which I love best I cannot tell.
Alas ! "—I cried, but checked the word,
For close behind a footstep heard
Compel'd me turn ; when, lo ! that Maid,
Dress'd in black velvet, who bewray'd
Plump Popes and Pastors once to fear,
Came up and took me by the ear.

"Is this the way," she cried, "you waste
Time should be spent in huddling haste
To harry Ignorance to her den,
Or pink fat Folly with the pen?
Small unobservèd things to use,
Each with its little mite of news,
To build that sheer hypothesis
Whose base on righteous Reason is.
Whose point among the Stars. For shame!
Enough the seeming-serious game.
But search the Depths; and for thy meed,
A place among the men indeed."

This rather tends to upset my own hypothesis; for I evidently thought in those youthful days that Science, clad in black velvet, was a much more serious dame than her sister Muses, and I was clearly rather sorry to be led away!

At that time (1881) I had just arrived in India, where, like other young men, I was much struck by the difference between the civilization there seen and that which I had just left, and was casting about in my mind to discover the cause. The following sonnet on "Thought" indicates something of this endeavour:—

THOUGHT.

Spirit of Thought, not thine the songs that flow
To fill with love or lull Idalian hours;
Thou wert not nurtured 'mid the marish flowers,
Or where the nightshade blooms, or lilies blow;
But on the mountains. From those keeps of snow
Thou seest the heavens, and earth, and marts and towers
Of teeming man; the battle smoke that lours
Above the nations where they strive below;

The gleam of golden cohorts and the cloud
Of shrieking peoples yielding to the brink—
The gleam, the gold, the agony, the rage;
The civic virtue of a race unbow'd;
The reeling empire, lost in license, sink;
And chattering pigmies of a later age.

Although European civilization was much more recent than that of the East, it was clearly superior in many ways, though not in all; if only as proved by the fact that a handful of Englishmen could, not alone maintain dominion over such a great country, but also give it innumerable benefits. I attributed this largely, and rightly, to our superior knowledge of Science. And in the following sonnet I attempted the praise of the great scientific Discoverers:—

Science.

I would rejoice in iron arms with those
Who, nobly in the scorn of recompense,
Have dared to follow Truth alone, and thence
To teach the truth—nor fear'd the rage that rose.
No high-piled monuments are theirs who chose
Her great inglorious toil—no flaming death ;
To them was sweet the poetry of prose,
But wisdom gave a fragrance to their breath.
Alas ! we sleep and snore beyond the night,
Tho' these great men the dreamless daylight show ;
But they endure—the Sons of simple Light—
And, with no lying lanthorne's antic glow,
Reveal the open way that we must go.

But if it was Science which had carried European civilization so far forward, there seemed to me still to be something, not Science, but rather the opposite of Science, which had retarded Indian civilization for centuries—because otherwise India should at least have kept abreast of Europe. This indeed is a very great problem, which our own science has scarcely yet begun to consider. We have no right to assume that civilization always advances ; often it stands still, and even recedes, owing to some decadence ; and it may recede in the Europe of to-morrow as it did in the India of yesterday. That is a terrible thought, which I put into the following verses, also written nearly forty years ago :—

India.

Here from my lonely watch-tower of the East
An ancient race outworn I see—
With dread, my own dear distant Country, lest
The same fate fall on thee.

Lo, here the iron winter of curst caste
Has made men into things that creep ;
The leprous beggars totter trembling past ;
The baser sultans sleep.

Not for a thousand years has Freedom's cry
The stillness of this horror cleaved,
But as of old the hopeless millions die,
That yet have never lived.

Man has no leisure but to snatch and eat,
Who should have been a god on earth ;
The lean ones cry ; the fat ones curse and beat,
And wealth but weakens worth.

O Heaven, shall man rebelling never take
From Fate what she denies, his bliss ?
Cannot the mind that made the engine make
A nobler life than this ?

In the concluding stanza my whole philosophy of life began to emerge, "Cannot the mind which made the engine make a nobler life than this?" In short, I invoked Science to heighten civilization and to prevent decadence. It is a philosophy derived from Epicurus, through Lucretius, Comte, and Spenser, and culminating in the high and pure philosophy of Science of to-day. Both a passive but also an active philosophy—one which studies, not only how things are caused, but also how they may be bettered. I have never liked the passive philosophies, most of which appear to me to be as untrue as they are useless; and in 1890 I tried to figure the difference in a discussion between the spirits of Philosophy and Science, or rather of passive and active philosophy:—

VISION.

Philosophy and Science.

A Valley of far-fallen rocks,
Like bones of mouldering mountains, spread,
And ended by the barren blocks
  Of mountains doom'd or dead:
No rivage there with green recess
Made music in that wilderness.

Despairing fell the sore-spent Sun,
And cried, "I die," and sank in fire;
Like conquering Death, the Night came on
  And ran from spire to spire;
And swollen-pale ascended soon,
Like Death in Life, the leprous Moon.

On windy ledges lined with light,
Between the still Stars sparsely strewn,
Two Spirits grew from out the Night
  Beneath the mistless Moon,
And held deep parley, making thought
With words sententious half distraught.

One full-robed; in his hand a book;
His lips, that labour'd for the word,
Scarce moved in utterance; and his look
  Sought, not his face who heard,
But that Sad Star that sobs alway
Upon the breast of dying Day.

One, weary, with two-handed stress
Leant on his shoulder-touching spear,
His beard blown o'er the hairiness
  Of his great breast; and clear
His eyes shot speculation out
To catch the truth or quell the doubt.

Philosophy.

"The dreams of Hope, of blue-eyed Hope,
Melt after morn and die in day;
Love's golden dew-globe, lit aslope,
Dulls with a downward ray;
Canst thou with all thy thought renew
The flying dreams or drying dew?"

Science.

"Not I creator. Hour by hour
I labour without stress or strife
To gain more knowledge, greater power,
A nobler, longer life.
By thought alone we take our stand
Above the world and win command."

Philosophy.

"Know, Knowledge doth but clip our wings,
And wordly Wisdom weaken worth,
To make us lords of little things,
And worm-gods of the earth.
Were earth made Heaven by human wit,
Some wild star yet might shatter it."

Science.

"The wings of Fancy are but frail,
And Virtue's without Wisdom weak;
Better than Falsehood's flowery vale,
The Truth, however bleak.
Tho' she may bless not nor redeem,
The Truth is true, and reigns supreme."

Philosophy.

"Not all, but few, can plead and prove
And crown their brows with Truth and pass;
Their little labours cannot move
The mountain's mighty mass.
To man in vain the Truth appeals,
Or Heav'n ordains, or Art reveals."

Science.

"So self-consuming thought. But see
The standards of Advance unfurl'd;
The buds are breaking on the lea,
And Spring strikes thro' the world.
Tho' we may never reach the Peak,
God gave this great commandment, Seek."

The ponderous bolts of Night were drawn;
The pale Day peer'd thro' cloudy bars;
The Wind awoke; the sword of Dawn
Flasht thro' the flying Stars;
The new-born Sun-Star smote the Gloom:
The Desert burst in endless Bloom.

The ideal here is the conquest of nature and the perfectibility of man and of society by Science. It may lead to a Utopia perhaps, but its Utopia, unlike some others, has the advantage of being practicable. It, like Milton's philosophy, "with no middle flight," intends to soar; but it does not intend to soar "above th' Aonian Mount" because indeed Truth can live no higher than there. And I therefore call it the Heliconian Philosophy—for reasons which will be indicated.

But it is the duty of those who hold this lofty creed themselves to work for the betterment of mankind. Now all this time, immersed in so many fine thoughts, I had somewhat neglected my finer duties, especially those concerned with that deity, Æsculapius, whom I had agreed, rather against my will, to follow. But, as I have said in the preface to my book of poems, "Philosophies," I now "began to be drawn toward certain thoughts which had occurred to me in my profession, especially as to the cause of the widespread sickness and of the great misery and decadence of the people of India. Racked by poverty, swept by epidemics, housed in hovels, ruled by superstitions, they presented the spectacle of an ancient civilisation fallen for centuries into decay." The following verses summarise what I mean:—

THE INDIAN MOTHER.

Full fed with thoughts and knowledges sublime,
And thundering oracles of the gods, that make
Man's mind the flower of action and of time,
I was one day where beggars come to take
Doles ere they die. An Indian mother there,
Young, but so wretched that her staring eyes
Shone like the winter wolf's with ravening glare
Of Hunger, struck me. For to much surprise
A three-year child well nourished at her breast,
Wither'd with famine, still she fed and press'd—
For she was dying. "I am too poor," she said,
"To feed him otherwise"; and with a kiss
Fell back and died. And the soul answerèd,
"In spite of all the gods and prophets—this!"

Led then by this duty, in 1890 I determined to devote myself to a thorough investigation of at least one of the great diseases referred to—malaria; but honesty compels me to add at the outset that this work was only of secondary interest to me, and that I undertook it as a duty and at considerable loss to myself. Personally I much prefer literature, mathematics and other studies, and am not a biologist, much less a medical man, by my natural proclivity. The following lines better indicate why I commenced the enterprise:—

INDIAN FEVERS.

In this, O Nature, yield I pray to me.
I pace and pace, and think and think and take
The fever'd hands, and note down all I see,
That some dim distant light may haply break.

The painful faces ask, Can we not cure?
We answer, No, not yet; we seek the laws.
O God, reveal thro' all this thing obscure
The unseen, small, but million-murdering cause.

But I did not know at the time that *seeking the laws* in this case would mean heavy toil for some seven years at least: and, still less, that success was to be obtained only by the most extraordinary good luck, without which we should, I think, have been still seeking in vain. Success was also due, I think, to the fact that the scientific work was first carefully designed, just as a work of art ought to be.

My own personal feelings during this long investigation—a series of disheartening failures until success was finally reached—have been set forth in my suite of verses called "In Exile," a part of my "Philosophies." It is a unique poem, I believe, in one respect, that it was written *pari passu* with a laborious scientific investigation, but it was not *intended* to be a scientific poem. On the contrary I proposed merely to give expression to my own summaries of things in India, as I experienced them one after the other. What happened was this, that most of the stanzas not connected with my malaria work were left unfinished, and that when these fragments were cast out the remainder took the form of an intense scientific drama, certainly not originally designed by me—a drama concerned with the life and death, not of a hero and heroine, but of millions of people. I think I should tell you this because it is connected with the subject of this lecture; but you would not thank me for attempting to declaim the drama to-night, and I will repeat only a few lines of it which bear especially upon our theme. The following describe the scientific spirit:—

IN EXILE.

I hold with them who see
  Nor only idly stand
The deed of thought to be
  Worth many deeds of hand.

Ever as we journey sink
  The old behind the new,
And Heav'n commands we think
  As justly as we do.

One golden virtue more
  Than virtue we must prize,
One iron duty more
  Than duty, to be wise.

Who to himself hath said,
  "This chamber must be closed;
This tract of truth I dread,
  This darkness God-imposed

May not be lifted," keeps
  An every-open door
Thro' which deception creeps
  Confounding more and more,

Until to wild extremes
  Of falsehood driv'n he dies,
Intoxicate with dreams
  And drunk with a thousand lies.

If with unshaken will,
  Resolving not to stray
But to be rising still,
  We clamber day by day

From truth to truth, at last,
  In valleys of the night
Not lost, we know the vast
  And simple upper light,

Only one labouring knows.
  The base, tumultuous wreck
Of rock and forest shows;
  The summit, a single peak.

So sought, so seen, so found.
  And what the end so high?
A summit splendour crown'd
  Between the earth and sky.

Where with sidereal blaze
  The mistless planets glow,
And stars unsully'd gaze
  On unpolluted snow.

No strife the vast reveals
  But perfect peace indeed—
The thunder of spinning wheels
  At rest in eternal speed.

The following sonnetelle (as I call the groups of three stanzas) was a first draft of one written on the 21st August, 1897, the day after that on which I first found the parasites of malaria in mosquitoes:—

This day relenting God
  Hath placed within my hand
A wondrous thing; and God
  Be praised. At His command,

Seeking His secret deeds
  With tears and toiling breath,
I find thy cunning seeds,
  O million-murdering Death.

I know this little thing
  A myriad men will save.
O Death, where is thy sting?
  Thy victory, O Grave?

I have not attempted to correct the poor technique of this first draft, but subsequently added two lines left blank in my note book. Perhaps the subject of the stanzas may excuse their imperfections! After all, that victory over a disease which slays every year many more than a million people may be thought worth commemorating in some kind of verse!

My book, "Philosophies," concludes with three lyrics, written in a special music of rhythm and euphony (which you may not like). I call them my pæans of victory.

Man.

Man putteth the world to scale
  And weigheth out the stars;
Th' eternal hath lost her veil,
  The infinite her bars;
His balance he hath hung in heaven
  And set the sun therein.

He measures the lords of light
  And fiery orbs that spin;
No riddle of darkest night
  He dares not look within;
Athwart the roaring wrack of stars
  He plumbs the chasm of heaven.

The wings of the wind are his;
  To him the world is given;
His servant the lightning is,
  And slave the ocean, even;
He scans the mountains yet unclimb'd
  And sounds the solid sea.

With fingers of thought he holds
  What is or e'er can be;
And, touching it not, unfolds
  The sealed mystery.
The pigmy hands, eyes, head God gave
  A giant's are become.

But tho' to this height sublime
  By labour he hath clomb,
One summit he hath to climb,
  One deep the more to plumb—
To rede himself and rule himself,
  And so to reach the sum.

Life.

From birth to death the life of man
  Is infinite on the earth,
To know and do that which he can
  And be what he is worth.

Our mortal life, however wrought,
  Eternity is indeed;
For every moment brings a thought,
  And every thought's a deed.

And that is so much infinite
  Which may be divided much;
And if we live with might and mirth
  Our human life is such.

For him who has not might and mirth
  That which is not now is never;
And he who can live well on earth
  Does live in heaven for ever.

World-Song.

O Vision inviolate, O Splendour supernal,
  We stand in Thy white light like lamps alit in day;
Before Thee, Omnipotent, in sight of Thy glory,
  Our countenance is witherèd like stars in the sun.

Before Thee our Symphonies are still'd into silence;
  Thy wisdom we wot not nor ever shall we know;
But from Thy high throne, O God, Thy voice and Thy
      thunder
  In utterance reiterate give glory and strength.

These songs are the integrations of my philosophy, which is, I hope, both true and useful. My integrations of human life and character, however, are contained in a romance, "The Revels of Orsera," commenced thirty years ago, and just published (as my other literary efforts have been) by Mr. Murray. In this I have tried to analyse character into its constituent elements, and to set forth each element by itself in apposition. I will leave you to disentangle that simplification! I add (as another advertisement) that I hope soon to publish some of my verses in phonetic spelling, because I think that our present spelling does not adequately render the rich and varied euphony of our language.*

Seven years ago, on the Greek Easter Sunday, I was in the Valley of the Muses, on Mount Helicon, and saw the ruins of the Temple of the Muses there. Beside us ran Hippocrene, the fountain which

* But I cannot explain in this lecture my ideals regarding artistry, as distinct from art.

gushed from the hoof-mark of the winged horse Pegasus; the birthplace of Hesiod was close at hand; and the summits of Parnassus glowed in the sunlight between the black rocks of the gorge. At that spot, and in the age which was perhaps the greatest in human history, not one but all the Muses were worshipped there, and the Heliconian Philosophy was born. But now the pillars of the temple are all tumbled to earth and only the ancient pavements remain. Even these are desecrated, for great tortoises and other reptiles sun themselves upon them; herds of hogs wander grunting and quarrelling over them; and, instead of the songs of the Muses, we hear legions of asses braying to each other across the valley. I hope you note the parable—which is not of my making! I have tried to put it and some of my previous arguments in this sonnet.

THE SIGNPOST.

Adventurous Stranger who dost dare to climb
Huge Helicon: Remember and mark well
What every several Muse may deign to tell,
If thou would'st hear Their symphony some time.
Not who hymn heaven always roll the rhyme—
Who scan th' unutterable stars, foretell;
But haply as each far-firèd pinnacle
Fumeth at sunrise sing Their song sublime.

But if thou be too proud thou shalt be thrown
To dismal valleys where foul fog distills,
And the thick tortoise clambers to the stone,
The root-uprouting hog his belly fills,
And asses bray their wisdom to th' eternal hills.

Some years later, that is, two and a-half years ago (December 16, 1917), I visited the oracle of Apollo at Delphi, two days after we had been torpedoed off the Island of Ithaca and the Rock of Sorrow; and in my imagination I asked the god what was the cause of the war. These verses, published soon afterwards, describe the circumstances,* and his reply:—

LINES.

Between the grim Leucadian Rock of Woe
(Where Sappho sank) and storied Ithaca,
At morning the deep-lurking Murderer smote us
Thrice, and the great ship groan'd and plunged. But soon
Our long lean War-Hounds, gnashing teeth of rage,

* We were torpedoed off Ithaca at 8 a.m., but escaped on our escorting torpedo boats. We then proceeded to attack the submarines with depth-charges because they could not escape in the land-locked bay. We brought up one by a depth-charge and then sunk her with shells, and rescued 18 of her crew. Two aeroplanes participated in the hunt, and we think we destroyed another submarine by gun-fire Our ship, the *Chateau Renault*, was sunk.

Scour round and round to seek him, and the sea
Boils with their charges, bursting far below.
Sudden upchurnèd like a wounded whale,
Th' Assassin sees the sun and meets his doom;
For now made visible he becomes our prey—
We rip him with swift shells—he sinks head-first.
Then all the classic sea is strewn with Huns—
Pink faces clamouring for our mercy, loud
To get what they give not: our soldiers rage
To let them drown, but still we drag them forth.
Two giant sea-planes, come too late to save us,
Acclaim our victory; and our crowded ships
Leap forward for the Delphian Shore.

* * * *

We reach'd the Delphian Shore, we climb'd the Cleft
Castalian. Ruin'd all the sacred grove,
And silent the stern faces of the rocks.
But seven eagles, circling through the gorge
Gave omen like the spirits of the gods
Departed; and, like questioners of old,
My doubtful heart demanded one reply:
Great god Apollo, why this dreadful strife?
And in the night the far-heard answer came.

Apollo's Reply.

I taught this lesson unto minds of worth
That man himself should be the god of earth,
Even like me, omniscient and bright,
In perfect wisdom, perfect beauty dight.
But man would not be taught
And, climbing higher, fell—
A fancied heaven sought
But reach'd a real hell,
Now in their folly school'd
They find what they desire—
By evil idols ruled,
To die in blood and mire.

At least, this is what Apollo would have said if he had deigned to answer at all! By the "evil idols" he would have meant, not only the Militarism and Politicalism which caused the war, but also the whole brood of that parent idol, Nescience or Non-Intellectualism. Men suffer chiefly because they still worship these false gods, and "despise those plain earthly teachers, Reason, Work, and Discipline." On the other hand, the figures of Apollo, Pallas Athène, and the Muses are the personifications of the great intellectual virtues which have raised us from the barbaric state. Ah, it is a long time since they were worshipped on those mountains and in those valleys of

ancient Greece! Is the world so much better since then—much wiser, much greater? I am not so very sure: but if so the advance has been due to them, and perhaps still more to the teaching of Him of Nazareth. In our Philosophy we shall obey them all; not this nor that, but all. The ideal may be distant, though haply not so distant as some may fear, and I cannot describe it here in detail. But the only ideal worth having is one which we can always approach, but never reach.

To sum up, then. Science and Poetry dwell together. We shall reach Truth by seeking Beauty, and Beauty by seeking Truth. Nor shall we attain one without the other, for they live hand-in-hand on those far-fired pinnacles. For me, at least, Poetry is not a mere matter of words, a temporary beat of the heart, a wandering strain, the altar-fume of a cult of cloistered criticism. It is a record and a monument for humanity, meant to endure until, as Shelley said, the future dares forget the past. It is the inscription of all experience, the record of all things seen, the tablet of the heart, the epitaph of suffering, the song of the thing done, the pæan of victory. Poetry is the breath of Action climbed to the summit; Thought on the peak; Philosophy in music more divine; the perfected utterance of the Human Spirit. On the peak, I say; for it is only there that Poetry is heard.

[R. R.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1920.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Russell John Reynolds, M.B. B.Sc.

was elected a Member.

His Highness Maharaj Rana Sir Bhawani Singh, K.C.S.I., Maharaj Rana of Jhalawar, attended the Meeting and was admitted a Member.

The Special Thanks of the Members were returned to Dame Alice Godman, D.B.E., for her munificent Gift of £1,000 for the Advancement of Science, in the name of her husband, the late Dr. Frederick Du Cane Godman, D.C.L. F.R.S., who was for Sixty-three Years a Member of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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London Society—Journal, May 1920. 8vo.
Marconi's Wireless Telegraph Co.—Year Book of Wireless Telegraphy, 1920. 8vo.
Mersey Conservancy—Report on Navigation of River Mersey, 1919. 8vo. 1920.
Meteorological Office—Daily Readings, Feb.-March 1920. 4to.
Meteorological Society, Royal—Journal, Vol. XLVI. No. 194. 8vo. 1920.
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North of England Institute of Mining Engineers—Transactions, Vol. LXIX.; Vol. LXX. Parts 1-2. 8vo. 1919-20.
Pharmaceutical Society of Great Britain—Journal, May 1920. 8vo.
Philadelphia, Academy of Natural Sciences—Proceedings, Vol. LXXI. Part 2. 8vo. 1919.
Photographic Society, Royal—Journal, Vol. XLIV. Nos. 4-5. 8vo. 1920.
Physical Society of London—Proceedings, Vol. XXXII. Part 3. 8vo. 1920.
List of Fellows, 1920. 8vo.
Rome, Ministry of Public Works—Giornale del Genio Civile, Feb.-March 1920. 8vo.
Röntgen Society—Journal, Vol. XVI. No. 63. 8vo. 1920.
Royal Engineers' Institute—Journal, Vol. XXXI. No. 6. 8vo. 1920.
Royal Society of Arts—Journal, May 1920. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XL. Part 1. 8vo. 1920.
Royal Society of London—Philosophical Transactions, A, Vol. CCXX. No. 580. 4to. 1920.
Proceedings, A, Vol. XCVII. No. 684; B, Vol. XCI. No. 638. 8vo. 1920.
Year Book, 1920. 8vo.
Royal United Service Institution—Journal, Vol. LXV. No. 458, May 1920. 8vo.
Salford, Borough of—Report of Museum and Libraries Committee, 1918-19. 8vo. 1920.
Sanitary Institute, Royal—Journal, Vol. XL. No. 5. 8vo. 1920.
South Africa, Union of, Department of Agriculture—Journal, 1920, No. 1. 8vo.
Sweden, Royal Academy of Sciences—Handlingar, Band LIV. LVIII. LIX. 4to. 1915-19.
Arkiv: Botanik, Band XV. 3-4; Kemi, Band VII. 4-5; Zoologi, Band XII. 1-2. 8vo. 1919.
Årsbok, 1919. 8vo.
Swiss Chemical Society—Helvetica Chimica Acta, Vol. II. Fasc. 3. 8vo. 1920.
United States Bureau of Standards—Scientific Papers, Nos. 356, 358, 361, 364, 365, 368, 370. 8vo. 1920.
Technologic Papers, Nos. 62, 148-151, 154-156. 8vo. 1920.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XIX. No. 2. 8vo. 1920.
Experiment Station Record, Vol. XLII. Nos. 2-4. 8vo. 1920.
United States Geological Survey—Bulletin, No. 700. 8vo. 1919.
Water Supply Papers, No. 458. 8vo. 1919.
Fortieth Annual Report, 1919. 8vo.
United States, Library of Congress—Report, 1919. 8vo. 1920.
United States Patent Office—Official Gazette, Vol. CCLXXIII. No. 2-Vol. CCLXXIV. No. 2. 8vo. 1920.
Washington, National Academy of Sciences—Proceedings, Vol. VI. Nos. 2-3. 8vo. 1920.
Biographical Memoirs, Vol. VIII. 8vo. 1919.
Yorkshire Philosophical Society—Annual Report, 1919. 8vo. 1920.

GENERAL MONTHLY MEETING,

Monday, July 5, 1920.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Ambrose Edmund Butler, J.P. M.Inst.Mech.E.
Mrs. R. de l'Hôpital.
William Arthur Merrett Smart, M.R.C.S. L.R.C.P. B.Sc.(Lond.)

were elected Members of the Royal Institution.

The Chairman announced that the Institution had received a Legacy of £5000 from the late Dr. Rudolf Messel, who was a Member for thirty years.

The Chairman announced the decease of Professor Auguste Righi, on June 8, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution by the death of Auguste Righi, Senator of the Kingdom of Italy; Professor of Experimental Physics in the University of Bologna; Ph.D. Fellow of the Royal Academy of Lincei, Rome; Foreign Member of the Royal Society; Honorary Member of the Royal Institution; recipient of the Matteucci Medal (1890); and the Grand Prize of 10,000 lire of the Academy of Lincei (1892); and many honours conferred by scientific academies throughout the world.

Auguste Righi was a pioneer in many branches of scientific progress during the last fifty years, and opened a new era of physical investigation. His name is associated with all the great questions of Natural Philosophy solved in recent years. He invented a new electrical influence machine, the Induction Electrometer, showing the composition of vibratory movements. He made far-reaching discoveries in Electricity and Optics, analysed the action of Heat and Magnetism on the Electrical Resistance of Bismuth, along with the Rotation of the Lines of Force in the Magnetic Field.

In later years, at the University of Bologna, from 1889 until the end of his life, he carried on a series of most important researches on Photo-Electric Dispersion, which constitute an epoch-making record in experimental enquiry. He also made important contributions to the Zeeman Effect; new developments in the Electron Theory; and the Phenomena of Electric Waves.

Author of many books published in Italy, one of which, entitled "Modern Theory of Physical Phenomena," was translated into English.

The Managers desire, on behalf of the Members, to express their deep sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Aeronautical Engineers, Institute of—Journal, Vol. I. No. 1. 4to. 1920.
Agricultural Society, Royal—Journal, Vol. LXXX. 8vo. 1919.
American Academy of Arts and Sciences—Proceedings, Vol. LV. No. 4. 8vo. 1919.

American Geographical Society—Geographical Review, March 1920. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXX. No. 6. 8vo. 1920.
Australia, Commonwealth Institute of Science and Industry—Science and Industry, Vol. II. No. 4. 8vo. 1920.
Batavia, Royal Observatory—Observations at Secondary Stations, Vol. VII. 1917. 4to.
Belgium, Royal Academy of Sciences—Bulletin, 1919, Nos. 9-12; 1920, Nos. 1-3. 8vo. 1920.
Mémoires, in 8vo. Second Series, Tome IV. Fasc. 1. 1920.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXVII. Nos. 15-16. 4to. 1920.
British Astronomical Association—Journal, Vol. XXX. No. 8. 8vo. 1920.
British Dental Association—Journal, Vol. XLI. Nos. 12-13. 8vo. 1920.
Cambridge Philosophical Society—Transactions, Vol. XXII. No. 19. 4to. 1920.
Canada, Royal Society—Proceedings and Transactions, Third Series, Vol. XIV. 8vo. 1920.
Chemical Industry, Society of—Journal, June 1920. 8vo.
Chemical Society—Journal and Proceedings, June 1920. 8vo.
Chicago, John Crerar Library—Twenty-fifth Annual Report. 8vo. 1920.
Colonial Institute, Royal—United Empire, Vol. XI. No. 6. 8vo. 1920.
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Athenæum, June 1920. 4to.
Author, July 1920. 8vo.
British Engineers' and Export Journal, June 1920. 8vo.
Chemist and Druggist, June 1920. 8vo.
Church Gazette, June 1920. 8vo.
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Engineer, June 1920. fol.
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Law Journal, June 1920. 8vo.
London University Gazette, June 1920. 4to.
Model Engineer, June 1920. 8vo.
Musical Times, June 1920. 8vo.
Nature, June 1920. 4to.
Nuovo Cimento, May 1920. 8vo.
Science Abstracts, May 1920. 8vo.
Wireless World, June 1920. 8vo.
Florence, Biblioteca Nazionale Centrale — Bollettino delle Pubblicazioni Italiani, June 1920. 8vo.
Franklin Institute—Journal, June 1920. 8vo
Gauthier-Villars et Cie (the Publishers) — Traité de la Lumière. Par C. Huyghens 8vo. 1920.
Geological Society of London—Quarterly Journal, Vol. LXXV. Part 3. 8vo. 1920.
Abstracts of Proceedings, Nos. 1057-1058. 8vo. 1920.
List of Fellows, 1920. 8vo.
Greek Bureau of Information — International Situation in Greece. By M. Venizelos. 8vo. 1920.
Greenock Philosophical Society—Fifty-ninth Annual Report, 1919-20. 8vo. 1920.
Radiation and Matter. By C. G. Barkla (Watt Anniversary Lecture). 8vo. 1920.
Horological Institute, British—Journal, June 1920. 8vo.
Illuminating Engineering Society—Illuminating Engineer, March 1920. 8vo.
Imperial Institute—Bulletin, Vol. XVII. No. 4. 8vo. 1920.
Life-Boat Institution, Royal National—The Life-Boat, May 1920. 8vo.
Annual Report, 1919. 8vo. 1920.

Lisbon Academy of Sciences—Boletin da Segunda Classe, Vol. XI. Fasc. 3; Vol. XII. 8vo. 1918–20.
Journal de Sciencias, Tomo II. 8vo. 1919.
Monumentos do Literatura Dramatica Portuguesa, Nos. 1 & 4. 8vo. 1919.
Various Publications. 8vo and 4to. 1913–19.
London County Council—Gazette, June 1920. 4to.
London Society—Journal, June 1920. 8vo.
Madrid, Real Academia de Ciencias—Revista, Tomo XVIII. Nos. 1–3. 8vo. 1920.
Anuario, 1920. 12mo.
Mechanical Engineers, Institution of—Proceedings, 1919, Oct.–Dec. 8vo. 1920.
Meteorological Office—Geophysical Memoirs, No. 16: Aids to Forecasting. By E. Gold. 4to. 1920.
Professional Notes, No. 8: Temperatures and Humidities in the Upper Air. By C. K. M. Douglas. 8vo. 1920.
Daily Readings, April 1920. 4to.
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Microscopical Society, Royal—Journal, 1920, Part 1. 8vo.
Monaco, Musée Océanographique—Résultats des Compagnes Scientifiques, Fasc. 52–53. 4to. 1919–20.
New Jersey, Department of Conservation—Annual Report, 1919. 8vo. 1920.
New York, Society for Experimental Biology—Proceedings, Vol. XVII. Nos. 3–4. 8vo. 1919.
New Zealand, Dominion of—Official Year-Book, 1919. 8vo. 1920.
Statistics, 1918, Vol. III 4to. 1919.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, March–April 1920. 4to.
Paris, Société Française de Physique—Journal de Physique, June 1919. 8vo.
Pharmaceutical Society of Great Britain—Journal, June 1920. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLIV. No. 6. 8vo. 1920.
Physical Society of London—Proceedings, Vol. XXXII. Part 4. 8vo. 1920.
Rome, Ministry of Public Works—Giornale del Genio Civile, April–May, 1920. 8vo.
Royal Canadian Institute—Transactions, Vol. XII. No. 2. 8vo. 1920.
Royal Engineers' Institute—Journal, Vol. XXXII. No. 1. 8vo. 1920.
Royal Society of Arts—Journal, June 1920. 8vo.
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Proceedings, A, Vol. XCVII. Nos. 685–686; B, Vol. CXI. No. 639. 8vo. 1920.
Stonyhurst College Observatory—Meteorological Observations, 1919. 8vo. 1920.
Swiss Chemical Society—Helvetica Chimica Acta, Vol. III. Fasc. 4. 8vo. 1920.
Tóhoku Imperial University, Sendai, Japan—Science Reports, Vol. IX. No. 2. 8vo. 1920.
United States Department of Agriculture—Experiment Station Record, Vol. XLII. Nos. 5–6. 8vo. 1920.
Journal of Agricultural Research, Vol. XIX. Nos. 3–5. 8vo. 1920.
United States Geological Survey—Bulletin, Nos. 639; 640 H, J; 641 H, I, K; 544, 647, 650, 657, 660 A, D, H, J; 666, 690 A; 692, 694, 696, 699, 710 C, D, E; 711 D. 8vo. 1919–20.
Water Supply Papers, Nos. 361, 362, 382, 394, 405, 416, 425 A; 450 B; 454, 455. 8vo. 1916–19.
Professional Papers, Nos. 94, 98 L, O; 106, 108 C; 115, 117, 125 C. 4to. 1917–20.
United States Patent Office—Official Gazette, Vol. CCLXXIV. No. 3 – Vol. CCLXXV. No. 2. 8vo. 1920.

GENERAL MONTHLY MEETING,

Monday, November 1, 1920.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Richard Birkett Brooks,
John Fitzgerald Dalton,

were elected Members.

The Secretary reported the decease of Professor Armand Gautier on August 27, and of Professor John Perry on August 4, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution and the World of Science by the death of Emile Justin Armand Gautier, Commander of the Legion of Honour, Grand Officer of the Crown of Italy, Membre de l'Institut, Past President of the Academy of Sciences, the Academy of Medicine and of the Chemical Society of France; Fellow of the Royal Academy of Lincei, Foreign Member of the Chemical Society of London, and Honorary Member of the Royal Institution.

Armand Gautier was one of the foremost French investigators in Biochemistry. In 1872 he became Director of the first Laboratory of Biological Chemistry in France, and in 1884 Professor of Medical Chemistry in the Faculty of Medicine. His well-known investigations on the putrefaction of proteins and in the products of Metabolism were epoch-making discoveries; his enquiries and studies on the detection of small quantities of poisonous materials distributed in nature, such as arsenic, led to the knowledge of the wide diffusion in nature of hitherto undetected bodies.

He was the Author of Cours de Chimie (in 3 vols., viz. Vol. I., Chimie Minérale; Vol. II., Chimie Organique; Vol. III., Chimie Biologique); Des Fermentations; La Chimie de la Cellule Vivante; Les Toxines Microbiennes et Animales; Leçons de Chimie Biologique Normale et Pathologique; Chimie appliquée à la Physiologie, à la Pathologie et l'Hygiène (2 vols., viz. Vol. I., Chimie appliquée à l'Hygiène: Chimie appliquée à la Physiologie (1st part); Vol. II., Chimie à la Pathologie); Le Cuivre et le Plomb dans l'Alimentation et l'Industrie au point de vue de L'Hygiène; l'Alimentation et les Régimes chez l'Homme sain et chez les Malades; Etudes générale des Eaux Potables; Sur las Sophistication et l'Analyse des Vins; Cent-vingt Exercices de Chimie pratique; Notices sur les Travaux Scientifiques de M. Armand Gautier; Exposé des Titres et Travaux Scientifiques de M. Armand Gautier; Cinquantenaire de la Société Chimique de France. His life work is embodied in his original communications to scientific societies, amounting to over six hundred.

The Managers desire to express to Madame Gautier and the family their sincere sympathy with them in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to place on record their sense of the loss the Royal Institution has sustained by the death of John Perry, LL.D. D.Sc. F.R.S., Past President of the Institution of Electrical Engineers and of the Physical Society, General Treasurer of the British Association, Author of "Spinning Tops," "Calculus for Engineers," "Applied Mechanics," and "The Steam Engine."

Professor Perry was a brilliant engineer and mathematician. From 1875 to 1879 he was Professor of Engineering in Japan, and was among those who introduced modern science to that country. In collaboration with Ayrton he communicated a large number of important memoirs to various scientific societies.

In 1881 he became Professor of Engineering and Mathematics at the City and Guilds of London Technical College, and held this office until 1896, when he was appointed Professor of Mechanics and Mathematics in the Royal College of Science. During the time of his appointment as Examiner in Mathematics to the Science and Art Department he took especial interest in improved educational methods, with beneficial results. He was also elected a Member of the Royal Institution in 1906.

The Managers desire to convey to the family the expression of their sincere sympathy with them in their bereavement.

The Special Thanks of the Members were returned to Miss Groves and Mr. Thorne for the valuable and interesting gift of Chemical Specimens belonging to the late Professor C. E. Groves; also to Dr. Thomas W. Dewar. M.R.I., for his Donation of £20 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute, Pusa: Bulletins, Nos. 93, 95. Memoirs, Vol. V. Nos. 5–6; Vol. VI. Nos. 1–2. 8vo. 1920.
Agricultural Journal, Vol. XV. Part 3. 8vo. 1920.
Kodaikanal Observatory: Report for 1917–1919. 4to. 1918–20.
Bulletin, No. 62. 4to. 1920.
Palæontologia Indica: New Series, Vol. VII. No. 1. fol. 1920.
Records of the Geological Survey, Vol. LI. Part 1. 8vo. 1920.
British Museum (Natural History)—Catalogue of Lepidoptera Phalænæ, Supplement, Vol. II., text and plates. 8vo. 1920.
Flora of Jamaica, Vol. IV. 8vo. 1920.
Summary Guide to the Exhibition Galleries. 8vo. 1920.
Economic Series: No. 1A, The House Fly, its Life History, etc.: No. 2, Furniture Beetles. 8vo. 1920.
Abbadia Observatory, The Director — Procès-Verbaux de l'Académie des Sciences de Paris, Tomes VII.-VIII. 1820–27. 4to. 1916–18.
Aberdeen Chamber of Commerce—Journal, July–Oct. 1920. 8vo.
Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta: Rendiconti, Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXIX. 1° Sem. Fasc. 6–12. Classe di Scienze Morali, Vol. XXVIII. Fasc. 7–10. 8vo. 1920.
Aeronautical Society, Royal—Journal, July–Oct. 1920. 8vo.
Allegheny Observatory—Publications, Vol. IV. No. 1. 4to. 1920.
American Academy of Arts and Sciences—Proceedings, Vol. LV. Nos. 5–7. 8vo. 1920.

American Geographical Society—Geographical Review, April Sept. 1920. 8vo.
Antiquaries, Society of—Archæologia, Vol. LXIX. 4to. 1920.
Proceedings, Second Series, Vol. XXXI. 8vo. 1920.
Asiatic Society, Royal—Journal, July–Oct. 1920. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXX. Nos. 7–8, 1920. 8vo.
Memoirs, Appendix to Vol. LXII. 1920. 8vo.
Australia, Commonwealth of—Science and Industry, May–July, 1920. 8vo.
Bulletin, No. 18. 8vo. 1920.
Bankers, Institute of—Journal, Vol. XLI. Part 7. 1920. 8vo.
Batavia, Royal Meteorological Observatory—Regenwaarnemingen in Nederlandsch-Indie, 1915, Deel II.; 1916, Deel. II. 8vo. 1916–18.
Observations, Vols. XXXVI., XXXVIII. 4to. 1916–20.
Belfast Natural History and Philosophical Society—Proceedings, 1918–19; 1919–20, No. 3. 8vo. 1920.
Belgium, Royal Academy—Bulletin, 1920, Nos. 4–6. 8vo.
Bemrose Publishing Co. (Publishers)—Chamber of Commerce Register, 1920, 2 vols. 8vo.
Bibliographical Society—The Library, New Series, Vol. I, No. 2. 8vo. 1920.
Bolton Chamber of Commerce—Bolton: its Trade and Commerce, 1919. 8vo. 1920.
Boston Public Library—Bulletin, Fourth Series, Vol. II. No. 2. 8vo. 1920.
Sixty-Eighth Annual Report of the Trustees, 1919–20. 8vo.
Botanic Society, Royal—Quarterly Summary, No. 5. 8vo. 1920.
Bowen, C. Winthrop, Esq.—The American Historical Review, April 1920. 8vo.
The New York Genealogical and Biographical Record, January 1920. 8vo.
Brewster, G. W., Esq. (the Author)—Relativity and the recent Astronomical Discovery. 8vo. 1919.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXVII. Nos. 17–20. 4to. 1920.
British Astronomical Association—Journal, Vol. XXX. Nos. 9–10. 1920. 8vo.
Memoirs, Vol. XXI. Part 4. 8vo. 1920.
List of Members, 1920. 8vo.
British Dental Association—Journal, Vol. XLI. Nos. 14–21. 8vo. 1920.
Cambridge Observatory—Report of Observatory Syndicate, 1919–20. 8vo. 1920.
Cambridge Philosophical Society—Transactions, Vol. XXII. Nos. 21–22. 4to. 1920.
Canada, Department of Mines—Memoirs, Nos. 115–117. 8vo. 1919.
Bulletin, No. 31. 8vo. 1920.
Preliminary Report on Mineral Production, 1919. 8vo. 1920.
Summary Report, 1919, Parts B, D, and G. 8vo. 1920.
Munitions Resources Commission Report. 8vo. 1920.
Carnegie Endowment for International Peace—Manual of the Public Benefactions of Andrew Carnegie. 8vo. 1920.
Carnegie Institution—Mount Wilson Observatory, Report of the Director, 1919. 8vo.
Contributions to National Academy of Sciences, Nos. 65–68. 8vo. 1920.
Contributions from Mount Wilson Solar Observatory, Nos. 171–182. 8vo. 1920.
The Interferometry of Reversed and Non-Reversed Spectra, Parts 1–4. By Carl Barus. 8vo. 1916–19.
Elliptic Interferences in Interferometry, Parts 1–3. By C. Barus. 8vo. 1911–14.
Experiments with the Displacement Interferometer. By C. Barus. 8vo. 1915.

Chemical Industry, Society of—Journal, July-Oct. 1920. 8vo.
Chemical Society—Journal and Proceedings, July-Oct. 1920. 8vo.
List of Fellows, 1920. 8vo.
Chemistry, Institute of—Journal and Proceedings, 1920, Parts 3-4. 8vo.
Chili, Instituto Meteorologico—Lluvias en 1918. 8vo. 1919.
Civil Engineers, Institution of—Proceedings, Vol. CCX. Part 1. 8vo. 1920.
List of Members, 1920. 8vo.
Clodd, Edward, Esq. (the Author)—Magic in Names. 8vo. 1920.
Colonial Institute, Royal—United Empire, Vol. XI. Nos. 7-10. 8vo. 1920.
Coventry Chamber of Commerce—Year-Book, 1920. 8vo.
Dunraven, The Earl of, K.P. C.M.G. (the Author)—The Crisis in Ireland. 8vo. 1920.
East India Association—Journal, Vol. XI. Nos. 3-4. 8vo. 1920.
Edinburgh Chamber of Commerce—Journal, July-Oct. 1920. 8vo.
Editors—Animals' Defender, Aug.-Oct. 1920. 8vo.
Athenæum, July-Oct. 1920. 4to.
Author, Oct. 1920. 8vo.
British Engineers' Journal, Aug.-Oct. 1920. 4to.
Chemical News, July-Oct. 1920. 8vo.
Chemist and Druggist, July-Oct. 1920. 8vo.
Church Gazette, July-Oct. 1920. 8vo.
Dyer and Calico Printer, July-Oct. 1920. 4to.
Engineer, July-Oct. 1920. fol.
Engineering, July-Oct. 1920. fol.
Journal of Physical Chemistry, May-Oct. 1920. 8vo.
Junior Mechanics, July-Oct. 1920. 8vo.
Law Journal, July-Oct. 1920. 8vo.
Model Engineer, July-Oct. 1920. 8vo.
Musical Times, July-Oct. 1920. 8vo.
Nature, July-Oct. 1920. 4to.
New Church Magazine, July-Oct. 1920. 8vo.
Nuova Cimento, June-Aug. 1920. 8vo.
Physical Review, June-Sept. 1920. 8vo.
Science Abstracts, June-Aug. 1920. 8vo.
Terrestrial Magnetism, Vol. XXV. No. 2. 8vo. 1920.
Wireless World, July-Sept. 1920. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LVIII. Nos. 292-293. Supplement, Vol. LVII. Part 2. 4to. 1920.
Evans, Lady, M.A. M.R.I. (the Authoress)—Lustre Pottery. 4to. 1920.
Faraday Society—Transactions, Vol. XV. Part 3. 8vo. 1920.
Florence, Biblioteca Nazionale Centrale—Bollettino delle Pubblicazioni Italiani, July-Oct. 1920. 8vo.
Franklin Institute—Journal, July-Sept. 1920. 8vo.
Gauthier-Villars et Cie. (the Publishers)—Les Animalcules des Infusions. Par L. Spallanzani. 2 vols. 8vo. 1920.
La Respiration et la Transpiration des Animaux. Par A. L. Lavoisier. 8vo. 1920.
Geneva, Société de Physique—Compte Rendu des Séances, Vol. XXXVII. No. 2. 8vo. 1920.
Mémoires, Vol. XXXIX. Fasc. 3-4. 4to. 1920.
Geographical Society, Royal—Journal, Vol. LVI. Nos. 1-4. 8vo. 1920.
Geological Society of London—Quarterly Journal, Vol. LXXV. No. 300. 8vo. 1920.
Geological Survey—Summary of Progress, 1919. 8vo. 1920.
Greek Bureau of Foreign Information—The Salonica Side Show. By V. J. Seligman. 8vo. 1919.
The Victory of Venizelos. By V. J. Seligman. 8vo. 1920.
Harlem, Musée Teyler—Archives, Ser. 3, Vol. IV. 8vo. 1919.
Verhandelungen, N.S., Dl. 20. 8vo. 1918.

Horticultural Society, Royal—Journal, Vol. XLV. Parts 2–3. 8vo. 1920.
Illuminating Engineering Society—Illuminating Engineer, April–July, 1920. 8vo.
Imperial Institute—Bulletin, Vol. XVIII. No. 1. 8vo. 1920.
Johns Hopkins University—American Journal of Philology, Vol. XLI. No. 2. 8vo. 1920.
Circulars, Vol. XXXIX. No. 3; Vol. XL. Nos. 1–2, 4. 1918–19. 8vo.
Studies, Series XXXVI. No. 4; XXXVII. Nos. 1–4. 8vo. 1918–19.
Kentucky, Department of Geology—Oil and Gas Resources of Kentucky. By W. R. Jillson. 8vo. 1919.
Linnean Society—Journal: Botany, Vol. XLIV. No. 297; Vol. XLV. No. 301. 8vo. 1920.
London County Council—Gazette, July–Oct. 1920.
London Society—Journal, July–Oct. 1920, No. 31. 8vo.
London University—Gazette, July–Oct. 1920. 4to.
Mechanical Engineers, Institution of—Proceedings, Jan.–April, 1920. 8vo.
Meteorological Office—Daily Readings, May–Aug. 1920. 4to.
Fifteenth Annual Report of the Meteorological Committee. 8vo. 1920.
Professional Notes, Nos. 10–12. 8vo 1920.
Southport Auxiliary Observatory Report, 1919. 8vo. 1920.
Meteorological Society, Royal—Journal, Vol. XLVI Nos. 195–196. 8vo. 1920.
Metropolitan Asylums Board—Annual Report, 1919–20. 8vo.
Metropolitan Water Board—Annual Reports. Nos. 14–17, 1917–20. 8vo. 1920.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias y Revista, Tome XXXV. Nos. 1–2. 8vo. 1920.
Microscopical Society, Royal—Journal, 1920, Part 2. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 368–377. 8vo. 1920.
Musical Association—Proceedings, 45th Session, 1918–19. 8vo. 1920.
National Physical Laboratory—Report, 1918–19. 8vo. 1920.
New South Wales—Census, 1916, Part 12. 4to. 1918.
New York, Society for Experimental Biology—Proceedings, Vol. XVII. Nos. 5–6. 8vo. 1920.
Nizamiah Observatory, Hyderabad—Reports for 1917–19. 8vo. 1919–20.
Nottingham Chamber of Commerce—Industrial Nottinghamshire, 1920.
Numismatic Society, Royal—Numismatic Chronicle, 1920, Part 1. 8vo.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, May–June, 1920. 8vo.
Paris, Société Française de Physique—Journal de Physique et le Radium, Serie VI. Nos. 1–2. 8vo. 1920.
Procès-Verbaux, 1919. 8vo. 1920.
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GENERAL MONTHLY MEETING,

Monday, December 6, 1920.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Charles Fabry (Marseilles),
Jean Perrin (Paris),

were elected Honorary Members of the Royal Institution.

MARINE BIOLOGICAL
WOODS HOLE
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MASS.

A. Chaston Chapman, F.R.S.
Norman Drayton Grinké-Drayton,
Wilfred James Hemp,
George Edward Nash, C.B.E.
Mrs. L. Pennington,
James Taylor, M.D.
James George Weir, C.M.G. C.B.E.
Richard Ernest Winkfield,

were elected Members.

The Secretary reported, That the Managers, at their Meeting held this day, had re-appointed Arthur Keith, M.D. LL.D. F.R.S., to be Fullerian Professor of Physiology for a further term of three years.

The following Lecture Arrangements Before Easter 1921 were announced :—

J. Arthur Thomson, M.A. LL.D., Professor, Natural History Department, University of Aberdeen. Six Lectures Illustrated, adapted to a Juvenile Auditory, on The Haunts of Life: 1. The School of the Shore; 2. The Open Sea; 3. The Great Deeps; 4. The Freshwaters; 5. The Conquest of the Land; 6. The Mastery of the Air. On Dec. 30, 1920 (*Thursday*); Jan. 1, 4, 6, 8, 11, 1921.

Sir Gerald P. Lenox-Conyngham, R.E. F.R.S. Two Lectures on The Progress of Geodesy in India. On *Tuesdays*, Jan. 18, 25.

Sir James G. Frazer, D.C.L. LL.D. F.B.A. F.R.S. M.R.I. Three Lectures on Roman Life (Time of Pliny the Younger); London Life (Time of Addison); Rural English Life (Time of Cowper). On *Tuesdays*, Feb. 1, 8, 15.

Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S. M.R.I., Fullerian Professor of Physiology. Four Lectures on Darwin's Theory of Man's Origin (in the Light of Present Day Evidence). On *Tuesdays*, Feb. 22; March 1, 8, 15.

Arthur Harden, D.Sc. F.R.S., Head of Biochemical Department, Lister Institute. Two Lectures on Biochemistry (Vitamins). On *Thursdays*, Jan. 20, 27.

W. A. Herdman, C.B.E. LL.D. D.Sc. F.R.S., Professor of Oceanography, University of Liverpool. Three Lectures on Oceanography (Great Exploring Expeditions; Problems of the Plankton; The Sea-Fisheries). On *Thursdays*, Feb. 3, 10, 17.

Frank Balfour Browne, M.A. F.R.S.E. F.Z.S., Lecturer in Zoology, University of Cambridge. Two Lectures on Mason Bees and Wasps. On *Thursdays*, Feb. 24; March 3.

George C. Simpson, D.Sc. F.R.S., Director, Meteorological Office. Two Lectures on Meteorology of the Antarctic. On *Thursdays*, March 10, 17.

Percy C. Buck, M.A. Mus.Doc., Director of Music in Harrow School (with Musical Illustrations by the English Singers). Three Lectures on The Madrigal (Rhythm; Key; Technique). On *Saturdays*, Jan. 22, 29; Feb. 5.

A. Fowler, F.R.S. Pres.R.A.S., Professor of Astrophysics, Imperial College of Science. Three Lectures on Spectroscopy (Experimental Spectroscopy; Regularity in Spectra; Celestial Spectroscopy). On *Saturdays*, Feb. 12, 19, 26.

Sir Ernest Rutherford, LL.D. D.Sc. F.R.S. M.R.I., Nobel Laureate, Cavendish Professor of Experimental Physics, Cambridge. Three Lectures on Electricity and Matter. On *Saturdays*, March 5, 12, 19.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Washington, National Academy of Sciences—Proceedings, Nos. 7-8. 8vo. 1920.
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Zoological Society—Proceedings, 1920, Part 3. 8vo.

WEEKLY EVENING MEETING.

Friday, March 26, 1920.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

PROFESSOR SIR J. J. THOMSON, O.M. LL.D. F.R.S. Pres.R.S. M.R.I.

The Scientific Work of the late The Rt. Hon. Lord Rayleigh, O.M. P.C. D.C.L. F.R.S.,

Prof. (1887-1905), Hon. Prof. (1905-1919) of Natural Philosophy. Royal Institution.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, January 16, 1920.

SIR J. J. THOMSON, O.M. M.A. LL.D. D.Sc. F.R.S. M.R.I.,
Professor of Natural Philosophy, R.I., in the Chair.

SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. M.R.I.,
Fullerian Professor of Chemistry.

Low Temperature Studies.

IN 1906* it was pointed out that a workable thermoscope could be constructed by taking advantage of the fact that charcoal saturated with air or other gases at a low temperature is very sensitive to radiant energy, and that the small increase of temperature produced in a 1 gramme bulb of charcoal by the approach of a candle flame expels sufficient gas to move a suitable index. As the response was obtained even though, in the forms of apparatus then employed (Fig. 1), the radiation had to traverse several thicknesses of glass before reaching the charcoal, it was inferred that a still more sensitive thermoscope could be constructed if the receptacle containing the charcoal were closed by a thin membrane of stretched india-rubber, which is more transparent to heat than glass. Such a membrane being practically impervious to most gases at low temperatures,† it appeared possible to make the receptacle air-tight.

This arrangement was tried with the charcoal in a metal capsule covered with a very thin clear membrane, as described in the diffusion experiments of 1915.‡ The tube from this cell led to a small liquid manometer. The cell was immersed in liquid air with the clear membrane directed vertically upwards, and the charcoal was allowed to saturate with clean air up to atmospheric pressure. Covering the membrane was a light metal shutter. When everything had become equilibrated, the shutter was lifted, thus exposing the cell to radiation from any source above and in line with the neck of the silvered vacuum vessel. With a Leslie cube at room temperature as source, an immediate response was obtained, sufficient to displace the index of the manometer to the limit of its scale in a few minutes. On replacing the shutter the opposite effect was obtained, as the charcoal re-absorbed the gas displaced by the dark radiation.

In addition to rubber membranes, plates of rolled silver chloride

* Proc. Roy. Inst., xviii. p. 445.
† Proc. Roy. Inst., xvii. p. 424; xxi. pp. 558, 816.
‡ Proc. Roy. Inst., xxi. p. 558.

and polished rock-salt were successfully employed, both these substances being very transparent to dark heat rays.

These results were demonstrated as follows :—

(1) *Heat transference through a thin membrane.* In the parallel beam from an arc lamp was placed an unsilvered glass spherical vacuum vessel, 5 to 6 inches diameter, filled with filtered liquid air. In the focus of the rays thus obtained pieces of black paper were readily ignited. The same result followed when a thin membrane of stretched india-rubber was interposed, as also with a plate of polished

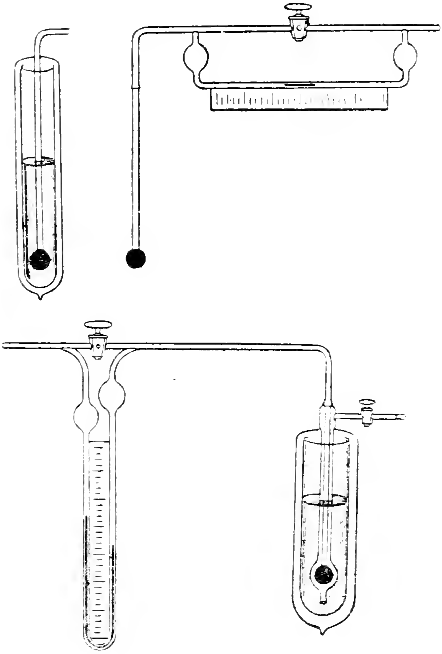

FIG. 1.

rock-salt. With an india-rubber membrane less stretched, and therefore thicker and not so clear, there was difficulty in getting the paper to glow, while a dull and still thicker membrane absorbed the bulk of the heat, and no appreciable effect could be obtained.

When a pad of wool soaked in liquid air was drawn across a stretched rubber membrane, puckering was produced by the expansion of the india-rubber. The opposite effect was demonstrated by means of an india-rubber balloon distended by water, and supported in a large glass funnel. By carefully pricking the balloon with a

fine needle, a steady jet of water was obtained, the upper limit of which was projected on the screen. On pouring warm water over the balloon, the resultant tightening of the rubber was shown by a sudden rise in the height of the water jet.

(2) *Elasticity of rubber membrane in liquid air and at ordinary temperatures.* An empty metal capsule 4 cm. diameter, covered by a membrane, was connected to a U-manometer, and its scale projected on the screen. A 5 cm. light watch glass rested on the membrane without appreciable distortion. When loads increasing up to 200 grammes were placed on the watch glass, corresponding displacements of the manometer were obtained, increasing uniformly up to 15 cm. pressure (alcohol). This experiment was repeated after cautiously and gradationally cooling the capsule in liquid air. The

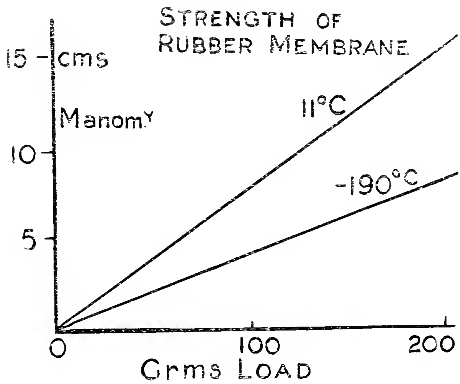

FIG. 2.

resulting displacements for the same loads reached only 8 cm., but were still quite regular, as shown in Fig. 2.

(3) *Membrane impervious at low temperatures.* This experiment was first shown in the Discourse of June 5, 1908.* An india-rubber membrane was stretched and bound over the end of a piece of glass tube about 2 cm. diameter, to which was sealed an ordinary discharge tube and charcoal bulb with stopcock. The membrane was cautiously cooled and immersed in liquid air, and the apparatus was exhausted by opening the charcoal stopcock. The vacuum was then too high for a discharge to pass, and remained so when the charcoal was shut off. When the membrane was cautiously warmed up by steadily lowering the liquid air vessel in which it was immersed, air began to pass through the membrane and the discharge started.

* Proc. Roy. Inst., xix. p. 417.

On re-cooling the membrane and opening the charcoal, the pressure was quickly reduced and the former high vacuum restored.

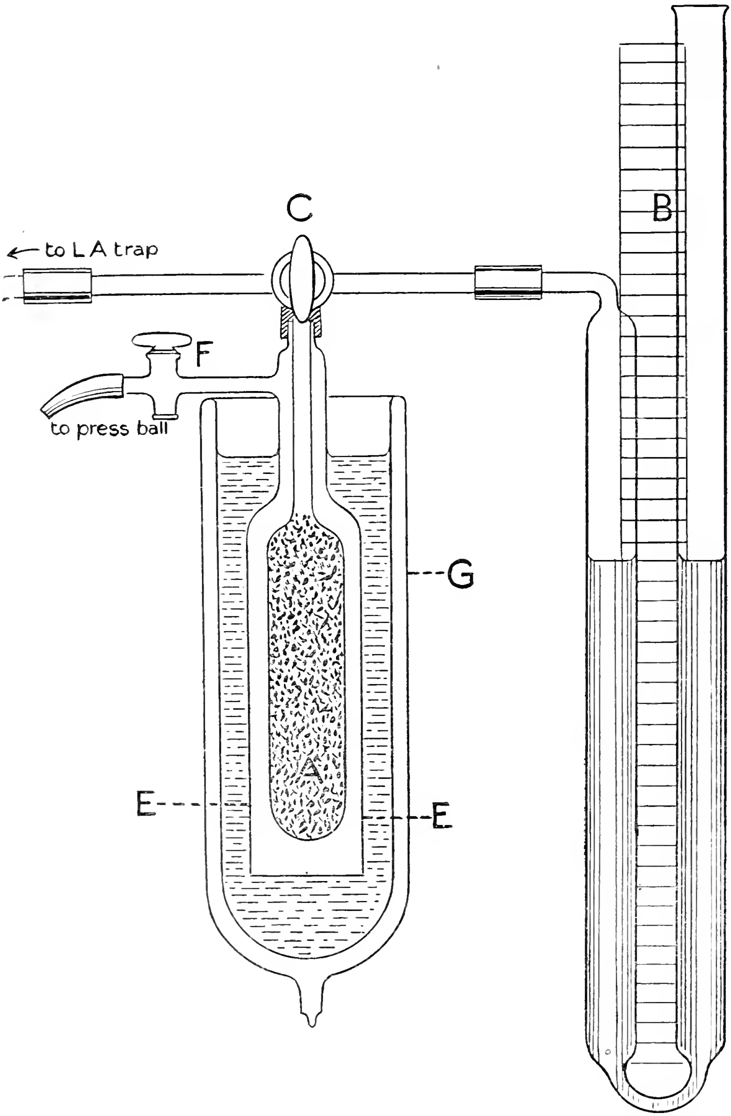

FIG. 3.

(4) *Action of a large charcoal thermoscope.* Instead of the small 1 gramme charcoal bulb (Fig. 1) and projected manometer shown on June 8, 1906, a cylindrical bulb A (Fig. 3), containing 30 grammes of charcoal, was connected to a manometer B, of which the limbs were 2 feet high. The bulb A was immersed in liquid air to saturate the charcoal to atmospheric pressure with clean dry air. A 3-way stopcock C at the top of the charcoal bulb A gave connection either to the manometer or to a liquid air trap (not shown) to clean the air before being condensed. An annular tube E, open at the bottom, covered the bulb, and was connected airtight above by rubber tubing. An opening with a stopcock F led to a press-ball, by means of which the liquid air could be forced out from the annular space round the charcoal bulb. The vacuum vessel G, in which the arrangement was immersed, was silvered half-way round, the remainder being clear; hence, when rotated, it screened off the radiation directed upon the charcoal from the front.

A dispersed beam 3 feet broad, from an arc lamp a yard away, was used to illuminate and actuate the thermoscope. With the silvered half of the vacuum vessel towards the lantern, and C turned to connect A to the manometer, the movement of the liquid was scarcely perceptible; but when the charcoal was exposed through the unsilvered glass, a displacement of 30 cm. in 15 seconds resulted, the annular space being full of liquid air. When this was emptied by the press-ball connected to F, the displacement in the same time was over 60 cm.

Measurements of Transmissive Powers.

In the cell employed for laboratory measurements the sensibility depends finally on the degree of relative isolation between the saturated charcoal and the liquid air or oxygen of the bath. If the charcoal is in close contact with the cell walls, the absorbed radiation is unable to raise the temperature of the charcoal sufficiently to affect the manometer to any extent; in the other extreme, not only are the initial displacements more erratic, but the time taken to re-equilibrate is too great. An arrangement that is very sensitive and quite workable is to lower the level of the liquid in the bath below the cell, but the space above the liquid must then have a very small temperature gradient. This condition can be secured by efficient cooling of the enclosing walls, as in the gas or air-cooled cell (see below).

The principal application of the arrangement was for measuring the relative absorptions of infra-red radiation up to 100° C., exhibited by a great number of substances when immersed in the form of plates of various thicknesses in the liquid air above the cell. The proportionate absorption was determined by two successive readings with the shutter raised for ½ to 1 minute to expose the cell to a

Leslie cube, the cell being unscreened for the first measurement, while for the second a plate of known thickness (down to a fraction of a mm.) of the material to be examined was placed on a light stage

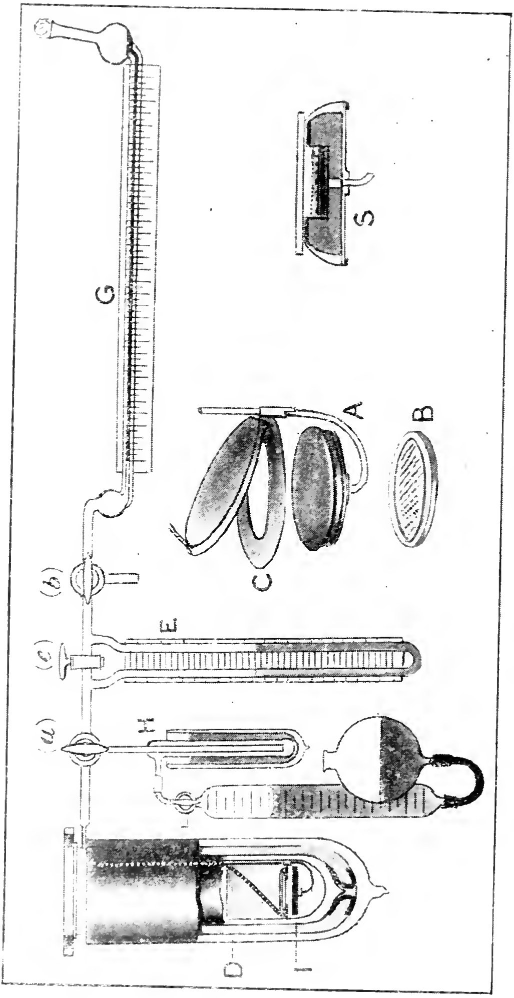

Fig. 4.—General Arrangement of Thermoscope.

above the membrane. Usually two sets of observations were made with the Leslie cube, one at room temperature and the other at 100° C. Some lower temperatures were tried with interesting results.

The working arrangement is shown in Fig. 4. Details of the

cell are shown at A. The metal capsule is covered by a transparent rubber membrane through which can be seen the tray of charcoal. This tray (shown separately at (B) below) was a tambourine of chiffon gauze, stretched by a pair of light brass rings of square section. A thin layer of clean dry absorptive charcoal—less than 1 gramme, in pieces of about $\frac{1}{2}$ mm.—was spread on the tambourine, which fitted loosely into the capsule; the charcoal was thus kept free in the cell space. The shutter above the cell was a simple hinged brass lid actuated by a light chain of silver or softened platinum wire, running through eyes and over a small pulley above (not shown). The lid, when closed, rested on a flat ring platform C, carried by a sleeve fitting on a thin (1 mm. bore, $\frac{1}{2}$ mm. walls) german silver tube leading out from the bottom of the cell. The plates of material whose transmissivity was to be measured were placed on the platform, the shutter being hollowed out sufficiently to allow this.

The form of the cell employed for plates of silver chloride or rock-salt is shown at S. The wall of the cell was a spherical segment of pure assay lead about $\frac{1}{2}$ mm. thick soldered to a brass disc as base, from which the thin german silver tube led to the manometer. The silver chloride (or rock-salt) plate was luted to the upper rim of the lead, which was flattened to receive it. The interior of this cell was occupied by an ebonite block, hollowed out above to contain the charcoal, as in the metal cell, and with a small opening below, to connect to the exit tube. The upper surface of the ebonite fitted under the lead rim, without being luted to it, as the contractions of lead and ebonite differ so considerably at low temperatures that otherwise there would be a tendency to crack the plate. Thus the lead rim was left free to follow the contraction of the rock-salt plate.

In a later form of cell the lead shell was soldered to a brass ring below, slightly coned to fit a similar ring on the periphery of the brass base plate. The ebonite was then used only for the first shaping of the lead rim, and the charcoal was supported in the lower half of the cell, as in the metal capsule already described. This arrangement allowed either charcoal or membrane to be altered as required without mutual disturbance.

The cell was immersed in liquid air (or oxygen or nitrogen, etc.) in the inner of two vacuum vessels (shown at D and I) arranged in the usual way for good isolation. The thin german silver tube led from the cell to a wider tube in connection with the manometers E and G. Two three-way stopcocks (*a*) and (*b*) were inserted on each side of manometer E, which itself had a plain stopcock (*c*) closed only when it was registering. The third connection of (*a*) was sealed to a liquid air trap H, of the usual annular form, leading to the supply of gas used to saturate the cell charcoal. With liquid oxygen or old liquid air in I, room air was good enough for this purpose; but nitrogen, hydrogen and helium were all employed in

testing the behaviour of the cell under different circumstances at different temperatures, as was also carbonic acid.

While the cell was being saturated, or settling between readings, stopcock (*a*) was set as shown—i.e. open to the gas reservoir at constant pressure, or open to the atmosphere; (*b*) was also in the same position as (*a*) open to the air (generally through a small drying bulb of fresh soda lime). When a reading was to be taken on E, (*c*) was closed, and (*a*) turned to connect the cell to the manometer only. A blank reading was then made, with the cell shutter closed, to test for equilibrium in the cell. The "zero" of the instrument, thus taken, did not exceed 2 or 3 mm. displacement, and usually was of the order of 1 mm. or less: (*a*) and (*c*) were then once more opened to equilibrate everything. In order to make an observation the cock (*c*) was closed, and the shutter raised before turning (*a*) to connect with the manometer E. (This was to avoid the fluctuations of pressure caused by eddies in the liquid due to the moving shutter and transmitted through the elastic membrane.) The stopcock (*a*) was then turned as before, and at the same moment the stopwatch was started. At the end of $\frac{1}{2}$ minute (or whatever the period required) (*a*) was reversed to open the cell to H, etc., and close the connection to the manometer E. The shutter was then lowered and the cell left for about 10 minutes to equilibrate. The manometer liquid remained in its displaced position until (*c*) was again opened, thus allowing ample time for the reading to be taken. In the same way for readings on the horizontal manometer G, (*b*) was turned to the position shown, with (*a*) set as first described; (*c*) remained open, and the reading was taken as before by turning (*a*) at the same time as the stopwatch was started, after raising the shutter.

In Fig. 5 are shown three alternative arrangements of cell and bath in more detail than is possible in Fig. 4. They consist essentially of inner and outer vessels (charged with liquid air or nitrogen, oxygen, etc.), the only purpose of the outer one being to arrest heat influx to the inner one, and thus to reduce to a minimum any fluctuations of the temperature of the cell immersed therein. The inner vessel of A, Fig. 5, with a straight central tubular outlet below was constructed of quartz, for the purpose of supporting the thin metal tube connecting the cell to the manometer; this arrangement left the whole space above the membrane free from obstruction. The vacuum isolation of this vessel did not extend up beyond two-thirds of its height, thus allowing the outer liquid air to maintain complete contact, and ensuring a uniform temperature throughout the inner vessel. The upper part should preferably be conical in shape, in order to cut off the radiation from the uncooled parts which otherwise would affect the cell when the shutter was raised. The simple shutter shown was lifted vertically for an exposure (the outer portions of the cell being covered with a ring diaphragm); but a properly hinged shutter was also used, fixed to a light sleeve fitting

into the vacuum vessel above the cell. An annular space filled with charcoal formed part of the construction of the quartz vessel, to ensure the maintenance of highest vacuum isolation at low temperature. Since the metal connecting tube of the cell passes through the liquid in the outer vessel, it is necessary that the liquid air used there should be of the same temperature and composition as that in the inner vessel. The safest procedure is to have liquid oxygen in both.

The simpler form shown in B was commonly employed, and was

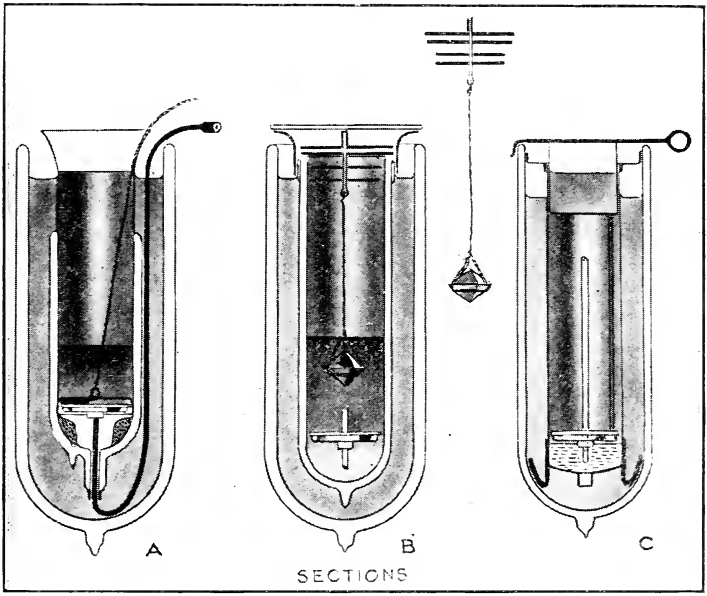

FIG. 5.—FORMS OF CELL.

the most simple to use for transmission measurements. Both inner and outer vessels were of the silvered cylindrical type. The isolation of the inner by the cooling of the outer was not so effective as in A, but was reinforced by the use of a set of thin polished metal discs with an axial support of an ebonite or thin german silver tube (closed at the ends). This device very effectively reduced the radiation and convection losses consequent upon the open unprotected neck. In addition a light sheet metal hood and cover plate were fixed to the neck to protect it from disturbance when opened. Attached by thread to the disc stopper was some uranium nitrate,

shown in the diagram as a single crystal, but small crystals were commonly used in a chiffon gauze bag. This substance becomes electrified at low temperatures with the slightest friction, and effectively removes floating crystals of ice or other impurities, thus keeping the liquid air perfectly clear. It becomes moist in use after some time, and care must be taken not to dehydrate it when drying, as it would then become inoperative.

The cell and tube are shown only in part (the tube being bent under and passing up behind the cell), and the trap door, etc., are omitted. With the most effective internal cooling, the trap door can be omitted, since the disc stopper, if well polished, almost completely cuts off external radiation. The drawback to this form is the stray radiation from the upper portion of the inner wall, whose temperature cannot be kept steadily down to that of the liquid bath. A lining of black bibulous paper or thin blackened sheet copper is of assistance, but does not eliminate the difficulty in such a vessel, which has to be opened intermittently during use.

Some of these drawbacks are eliminated in C, which was afterwards developed almost into a standard form. The inner vessel holds a separate small quantity of liquid, but being of plain metal is unisolated, and takes throughout the uniform temperature of the liquid in the outer vessel, which extends farther above the top of the inner vessel than the diagram indicates. The top is closed by a light metal cup fitting with the minimum of play; into this is placed some of the same liquid air as is used in the vessels. A uranium bag can be attached below, as with the disc stoppers, but in the figure the cell is shown "gas cooled," i.e. above the liquid in the inner vessel. A light cross support serves as a handle to remove this "bucket" stopper for an exposure. It is of course supported meanwhile in a suitable glass vacuum vessel of liquid air. The objection to this form, apart from the need of rather frequent replenishment of the bucket, is the cloud which sometimes forms when the atmosphere is moist, or if the bucket is lifted too rapidly. But with careful use very good results are obtained.

Curves of Displacement of Gas with Time.

The evolution of air or gas from the cell, as revealed by time and displacement curves, showed only a small departure from a linear rate for 2 or 3 minutes. After this the gas was expelled more slowly, and in from 10 to 15 minutes the manometer had almost stopped. Two manometers were used. The first was the ordinary U-tube form, to register the rate of growth of pressure; while in the second the rate of gas evolution at sensibly constant pressure was indicated on a horizontal scaled tube, with a bulb reservoir at the end furthest from the cell. In this form the linear character of the earlier displacement range continued longer than the ordinary U-tube, and

the time taken for settling, after an exposure. was less, since the gas equilibrium in the charcoal was not so much disturbed as when the pressure was raised.

The relative displacements resulting from the "gas-cooled" cell, as compared with those from the "liquid-cooled" cell (cell all

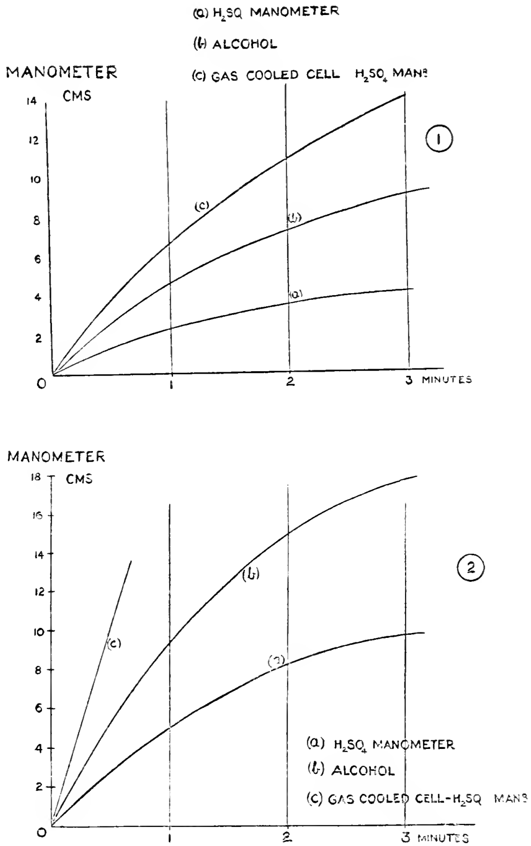

FIG. 6.—VERTICAL MANOMETER, 1, 15° C. RADIATION; 2, 100° C. RADIATION.

immersed) are included in the curves, Figs. 6 and 7, which show how the readings grow with continued exposure. Separate curves for radiation from a Leslie cube at 15° C. and 100° C. respectively are given, also comparative readings with either alcohol or sulphuric acid in the manometers. (Later petroleum was used, fractionated

between 125° C. and 140° C., and tinted with "scharlach-R." This, though as light and mobile as the alcohol, gave no vapour which by condensation might choke the narrow tubes of the thermoscope; the more viscous and dense sulphuric acid could then be dispensed with.) Half to 1 minute was the usual period of exposure when comparing fluxes of radiation, the displacements with the same aperture in

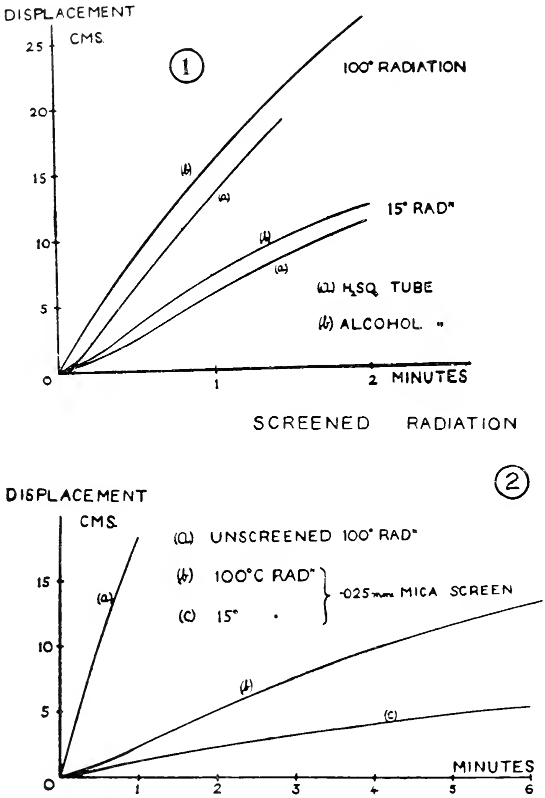

Fig. 7.—Horizontal Scaled Tube.

equal times being then compared; when the inertia of the liquid caused any initial distortion, the first 5 or 10 seconds were not counted. A longer period was advisable when a very absorptive screen gave low readings. The curve (Fig. 7 (2)) shows this in the case of mica, with which readings were continued for several minutes without the occurrence of serious bending in the plotted results, as the volume of gas expelled was still quite small. In this way the

necessary accuracy, which 1 minute readings would not secure, was attained. The time taken to re-equilibrate after such a reading is no longer than is necessary after an ordinary ½ to 1 minute's exposure to unscreened radiation, as the volume of gas expelled is no greater.

It was not intended, with such a cell, to get absolute measurements, because, as already seen, the charcoal is only partially isolated; hence only comparative measures were taken, everything being related to the unscreened radiation from the black Leslie cube at ordinary temperature, taken on each occasion for comparison.

When exposed, therefore, to a uniform flux of radiation, the absorbed gas will be liberated up to a steady limit defined by the small maintained increment of temperature of the charcoal above that of the bath. Directly the radiation is shut off, by lowering the

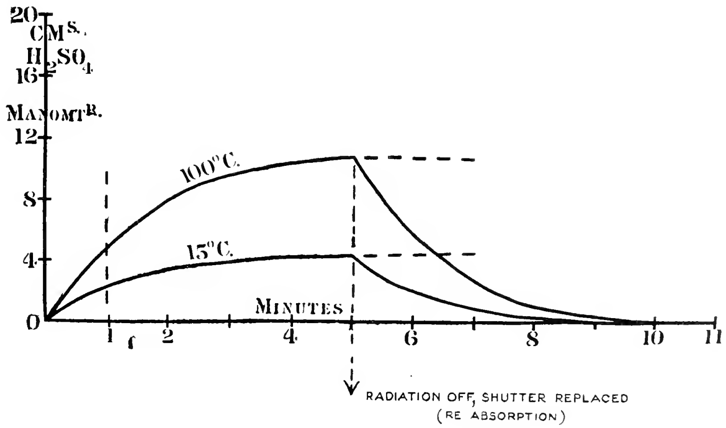

Fig. 8.

shutter over the cell, the charcoal again falls to the original temperature, and the expelled gas is re-absorbed. The general character of the curve is shown in Fig. 8.

An approximate idea of the possible sensibility may be gathered from a knowledge of the latent heat of gases in cooled charcoal. Thus a measurement made with 300 cc. of oxygen in 1 gramme of charcoal gave for a lowering of temperature from 90·23° Abs. to 83·2° Abs. a change of occlusion pressure from 1·0634 mm. to 0·2029 mm., i.e. a dp of 0·8605 mm. for a dT of 7·03°, or 0·123 mm. Hg. per 1°. This on a sulphuric acid manometer would be 0·905 mm. per degree.

Assuming, then, that the latent heat of the oxygen in charcoal would, at the saturation pressure of one atmosphere, be the same at

the same temperature as at the lower pressure of 1·06 mm., we should have—

$$\frac{T^2}{p}\frac{dp}{dT} = \frac{T^2}{p'}\frac{dp'}{dT}, \text{ giving } \frac{dp'}{dT} = \frac{p'}{p} \times 0\cdot905 = \frac{760}{1\cdot06} \times 0\cdot905$$

= 648·8 mm. of sulphuric acid per degree.

Since the indication of the sulphuric acid manometer with the charcoal thermoscope was frequently of the order of 3 cm. for ½ minute's exposure to a Leslie cube at 15° C., the corresponding rise of temperature would therefore be 0·05° Abs.

The measured evolution of gas at constant pressure on the horizontal scaled tube under the same circumstances was about 0·4 cc. Now, taking the thermal evolution of oxygen in charcoal as 3146 calories per gramme molecule,* corresponding to approximately 7·2 cc. evolved per calorie absorbed, the thermoscope registered 1/20th of a calorie for ½ minute's exposure. This corresponds approximately to 56·6 mm. displacement in the scaled tube of 3 mm. bore, so that each mm. represented approximately 0·001 calorie. For more exact measurements of smaller transmissions a tube of less diameter was used.

The maximum evolution of gas corresponding to the theoretical total emission from the black body was not usually obtained, but the proportion registered was higher with small disturbances and lower with exposures to more intense radiations. Two typical cases may be quoted. In the first, the cell was exposed in the ordinary way at the lower end of a cylindrical vacuum vessel with black absorptive walls; while in the second, the walls were made reflecting with a lining of thin polished metal. In the first case the cell was distant 18 cm. from the open neck covered by a black cube at 15° C. Now, with cell 2½ cm. diameter (4·9 cm.^{2} area) 18 cm. below the neck, of diameter 5½ cm. (neck area = 23·7 cm.^{2}), theoretical calories diffused through vessel from a black cube at 15° C., covering the whole open neck, and absorbed at 90° Abs. (see Fig. 13):

$= 1\cdot374 \times 10^{-12}\,(288^4 - 90^4)$

$= 0\cdot00945$ calorie per second per cm.^{2}.

Therefore total flux from exposed area of 23·7 cm.^{2} = 0·2245 calorie per second, or 13·47 calories per minute.

These 13·47 calories per minute may be regarded as spread out over a hemisphere of 18 cm. radius, i.e. over an area of 2036 cm.^{2}, of which surface the cell occupies 4·9 cm.^{2}; and will therefore absorb 4·9/2036 of the total radiation, viz. 0·032 calorie per minute. If

---

* Derived from a Rankine formula $\log p = A - B/T$, where B was found by experiment to be 684, whence the value of $\frac{T^2}{p}\frac{dp}{dt} = 2 \times 684 \times \log_e 10 =$ 3146. (Proc. Roy. Inst., xix. p. 416.)

all this is utilised by the charcoal as latent heat of evaporation of the absorbed air (approx. 8 cc. evaporated per calorie) the volume of gas evolved would be 0·256 cc.

In the same way the theoretical amount from the same black cube at 100° C. would be 38 calories per minute, entering the neck, of which the cell could absorb 0·091 calorie, equivalent to a gas evolution of 0·784 cc. In both cases the absorption in the membrane over the charcoal reduces this to approximately 80 per cent. The maximum gas evolution would therefore be of the order of 0·63 cc. per minute with the black cube at 100° C., and 0·22 cc. with the cube at 15° C. The observed values in one case were 0·34 cc. at 12° C. and 0·956 cc. at 100° C. There was, however, some additional radiation from the upper region of the walls of the vacuum vessel which the cooling liquid did not reach.

When, instead of a black lined vacuum vessel, a polished lining was used, practically all the radiation should reach the cell —namely, 2·68 calories per minute from a cube at 15° C., and 7·58 calories from a cube at 100° C., equivalent respectively to 21·44 cc. and 60·65 cc. of evaporated oxygen per minute. This however should be reduced, not only by the 20 per cent. absorption of the membrane, but also by at least 15 per cent. loss on reflection from the metal (assuming only single reflection, whereas much of the radiation will be several times reflected before reaching the lower end of the cylinder). This would give a maximum theoretical evolution of 14·6 cc. per minute from the cube at 15° C., and 41 2 cc. per minute from the cube at 100° C. This evolution would disturb the gas equilibrium in the cell too much, but 10 seconds' exposure gave measures at the rate of 3·8 cc. per minute for the 15° C. exposure and 9·0 cc. per minute for the 100° C. exposure— i.e. about one-fourth of the theoretical. This sufficiently indicates the increasing lag with increasing intensity.

The response of the cell to known small increments of temperature afforded another means of studying this question. A simple method was to increase by a small measured amount the pressure under which the liquid air was evaporating from the bath in which the cell was immersed. A manometer, similar to that employed for the radiation measurements, was connected to the closed vacuum vessel, and an outlet tube was arranged at different measured depths in an open vessel of mercury, sulphuric acid, or alcohol, according to the plus pressure required. In this way an increase of temperature of the liquid air amounting only to a few hundreths of a degree was obtained, and the consequent rise of temperature of the cell was observed by the movements of the manometer attached to it. The equilibrium of different gases in the charcoal was also studied, and information obtained as to the application of oxygen, nitrogen, hydrogen, helium, etc., to the purposes of a low temperature charcoal thermoscope.

A fairly direct calibration of the instrument can thus be made in terms of actual temperature increase. Since at the boiling point of liquid oxygen the dp/dt per 1° is 81 mm. Hg., an added pressure of 14 to 15 mm. of alcohol (0·8 mm. Hg.) would correspond to an increment of temperature of the liquid air or oxygen of 1/100th degree. The curves (Fig. 9) give the indications of the thermoscope following increments of pressure of 5, 10, 15, and 20 cm. of the alcohol manometer, the corresponding increments of temperature of the liquid oxygen being 0·023°, 0·067°, 0·10°, and 0·133° Abs. The response of the thermoscope with time after these instantaneous additions of pressure to the oxygen bath are seen to be of the same character in the case of air-saturated charcoal as those produced

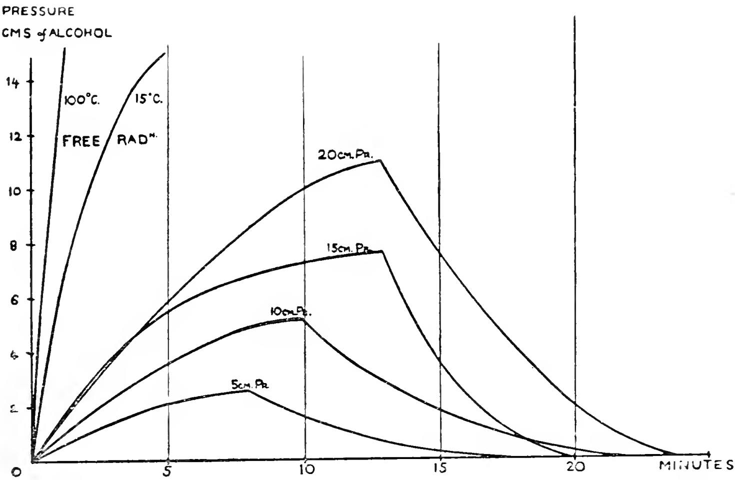

FIG. 9.—AIR SATURATION.

by the usual exposure to the Leslie cube, but of much less intensity, as shown by the comparison curve of the ordinary exposure. The limit registered by the thermoscope was in all cases less than that of the bath, as was to be expected from the higher latent heat of the air-in-charcoal as compared with the liquid oxygen. Thus the 5 cm. increment (0·033°) caused a rise of the thermoscope manometer to 2·6 cm. in 8 minutes, which on releasing the pressure of the liquid oxygen fell to 1 cm. in $3\frac{1}{2}$ minutes, and took 9 or 10 minutes to re-equilibrate to zero. Similarly for the other increments of 10 cm., 15 cm. and 20 cm. More extended measurements on these lines would no doubt give much information concerning the latent heat of gases in charcoal under various conditions.

With hydrogen saturating the charcoal instead of air (Fig. 10)

the limit was reached under the same circumstances in about $1\frac{1}{2}$ minute, and with helium (same figure), which of course is scarcely condensed at all at this temperature, the response was almost all registered in $\frac{1}{2}$ minute. These two results show incidentally that the temperature lag of the bath must be very small. For comparison here, also, the corresponding normal thermoscope responses to the Leslie cube are shown in separate curves.

All these results were the same, whether made on the membrane cell or on the silver chloride cell, so that no distortion of the

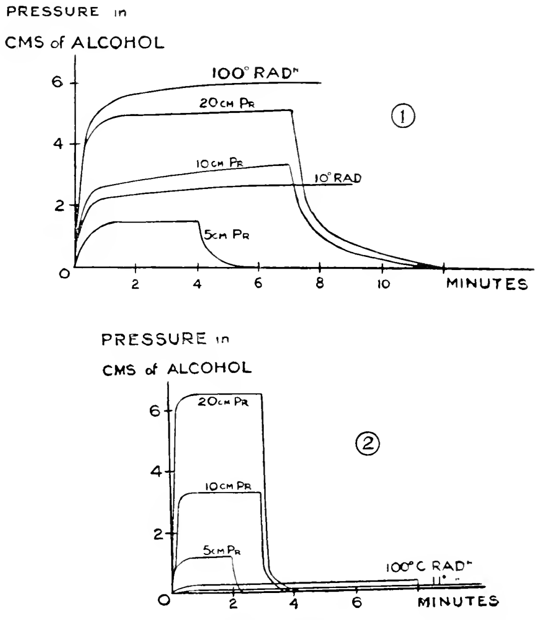

Fig. 10.—Charcoal Saturated with (1) Hydrogen, (2) Helium.

membrane under small differences of pressure will explain them. On the other hand, if no charcoal is present in the cell some effect can be detected from this cause, but the response is in all cases registered instantaneously. The curves in Fig. 11 give this response for different increments up to 20 cm. This pressure was calculated to correspond to a load of 180 grammes weight on the membrane. The response of the manometer, on the other hand, due to the stretching of the membrane, corresponded to a load of only 72 grammes, or a pressure of 8 cm., and so on for lower values. In this figure is also included the response obtained by an ordinary exposure, which

is seen to be negligible. Comparison distortions of the membrane by weight at ordinary temperatures and in liquid air have already been described.

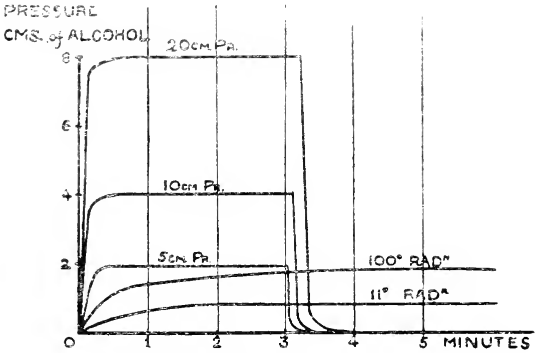

FIG. 11.—CELL WITHOUT CHARCOAL.

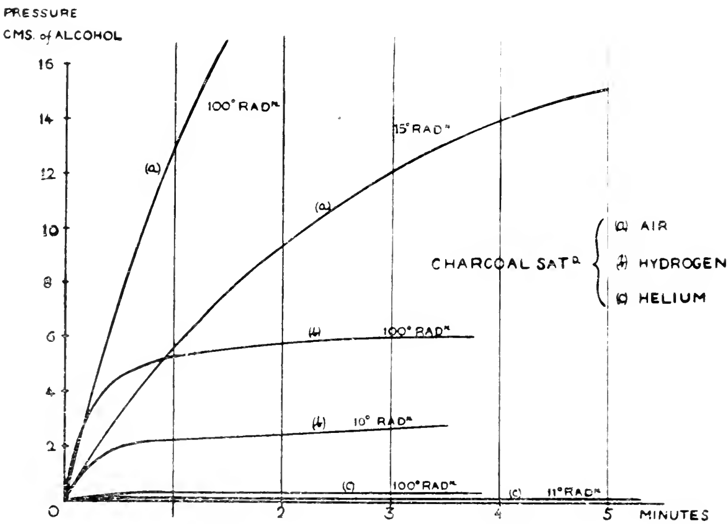

FIG. 12.

Curves comparing the sensibilities of the cell when the charcoal is saturated at liquid air temperature with air, hydrogen and helium respectively are collected in Fig. 12.

Comparison with Radiation Laws.

As already stated, the observed proportions between the manometer readings in some unit period were the basis on which the relative amounts of radiation transmitted were estimated. In order to test the validity of this interpretation, the results so obtained were compared with the theoretical values resulting from applying the fourth power law of Stefan. The ratios from black bodies at T_1 and T_2 respectively into a liquid oxygen "sink" at 90° Abs., are thus expressed as $\dfrac{T_1^4 - 90^4}{T_2^4 - 90^4}$.

Such ratios for certain common values of T_1 and T_2 are set out in Table I. Typical temperatures are given in columns 1 and 2, and the calculated ratios in column 3, while column 4 contains the average values obtained from the thermoscope measurements. These

Table I.—$(T_1^4 - 90^4)/(T_2^4 - 90^4)$ gives the Relative Energy Flux from Two Temperatures, T_1 and T_2, into Liquid Oxygen "Sink."

T_1	T_2	Relative Flux	
		Theory	Observed
100° C.	15° C.	2·83	2·6
100 ,,	−78 ,,	14·0	12·9
15 ,,	78 ,,	4·94	4·8
100 ,,	0 ,,	3·52	3·4
0 ,,	−78 ,,	3·98	3·7

ratios may be deduced directly from such a curve as that given in Fig. 13, where the values of $(T^4 - 90^4)$ are given as ordinates with T as abscissa up to T = 373° Abs., the higher position of the curve being shown separately with scale of ordinates to the right.

For test observations all stray radiation into the cell was either reduced as far as possible by screens or else directly measured and corrected for, by covering the vessel with a basin containing liquid air and making a "blank" exposure. For ordinary measurements of transmissivity through various materials a little stray internal radiation was not regarded as serious, as it had all to pass through the material under examination before being registered. The proportional transmission of the whole mixed radiation was therefore recorded. The principal temperatures used for the Leslie cube (or its equivalent) above the vessel (for the test measurements) were boiling water, room temperature of 10° to 15° C., melting ice and solid carbonic acid finely pounded, moistened with alcohol and

settled to a steady boiling point in an enclosed vessel. (It is very easy for solid carbonic acid to fall to a temperature considerably below its boiling point, by forced evaporation due to mere exposure to the strong air convection currents arising from free exposure of such a cold substance.) Besides this, a set of measurements at gradationally lowered temperatures between 0° and − 78° C. was made, using a cooled alcohol bath as source. Measurements much above 100° C. were not satisfactory—e.g. the use of a source of boiling sulphur produced too violent a disturbance and did not give satisfactory values; but, on the other hand, the difference between old

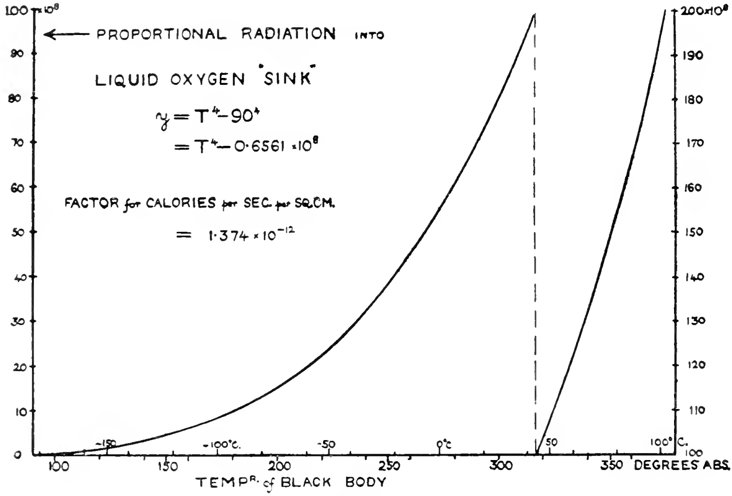

Fig. 13.

and new liquid air, say at 89° Abs. and 82° Abs. respectively, was easily registered, either plus or minus as the case might be; and also that between boiling oxygen and boiling nitrogen (90° Abs. and 77° Abs. respectively). No observations were made with liquid hydrogen, either as "black" source (negative) or to cool the thermoscope (hydrogen saturated charcoal), with a suitable Leslie cube of liquid oxygen or nitrogen as black body above, but there is no reason to doubt that this could be arranged satisfactorily. Among other questions the transmissivity for heat of liquid hydrogen could thus be tested.

Typical Results of Transmissive Power.

Table II. exemplifies the simplest manner of deducing the proportionate transmissions of typical substances. These substances are enumerated in the first column, and their thicknesses in the second; the third and fourth columns give the actual displacements of the manometer, in two sets, with the "black body" at 15° C. and 100° C., and the fifth column gives the proportionate transmissions, based on the readings given in the first line with unscreened radiation. The first two substances were equally transmissive at several thicknesses.

Table II.—Liquid Air Thermoscope Observations.

Solids	Mm.	Manometer 15° C.	Manometer 100° C.	Transmissions
		cm.	cm.	per cent.
—	—	4·8	12·7	100
Rock salt . . .	3·8	4·4	11·7	92
Silver chloride .	1·0	4·1	10·8	85
	0·15	4·2	10·9	86
Rubber . . .	0·6	0·2	0·5	4
	0·1	1·6	4·2	33
	0·02	3·75	10·0	78
Crystalline quartz .	2·3	0·75	2·0	16
Glass . . .	1·0	0·5	1·3	10
	0·125	0·9	2·3	18
	0·012	1·8	4·7	37
Mica	0·10	0·2	0·5	4
	0·062	0·4	1·0	8
	0·013	1·6	4·2	33
Fused quartz . .	1·5	0·55	1·5	12

The next, rubber, became very absorptive by the time a thickness of 0·6 mm. was reached, although very transmissive when stretched to the form of a transparent membrane 0·02 mm. thick. Crystalline quartz 2·3 mm. thick was more transmissive than fused quartz of 1·5 mm.—viz. 16 per cent. and 12 per cent. respectively. Glass, even thinner than the rubber membrane, was still able to absorb two-thirds of the incident radiation, and the absorption of mica increased at an even more rapid rate with thickness. In this connection very thin films of water and alcohol, two very absorptive bodies, were found to be quite fairly transmissive, as were also collodion films, especially when thin enough either to appear black or to show the first order colours by reflected light. The thin liquid films were measured between polished plates of silver chloride, while the collodion films were picked up by rings from the surface of water.*

* Proc. Roy. Inst., xxi. p. 787.

A great number of bodies were studied. Those solid at ordinary temperature were usually ground and polished to the requisite thickness. If not obtainable in pieces of sufficient size, the powdered or crystalline material was compressed hydraulically into suitable plates, sometimes with the addition of a trace of solvent. Fusible substances were usually cast. Mica laminæ were stripped in the usual way; while glass fragments of sufficient size ($2\frac{1}{2}$ cm. disc) but thinner than micro-cover glasses were selected from large thin bulbs. The majority of observations were, however, made on materials liquid at the ordinary temperature, although solid when cooled in liquid air; and various devices became necessary to get suitable plates of these for immersion above the cell. Several could, by careful partial cooling, be rendered sufficiently viscous to give a coherent disc when poured on a quartz plate supported above a liquid air surface. By further gradational cooling the viscous disc could very often be solidified to a clear glass usually quite readily separable from the quartz plate. The large number of substances which give clear glasses when properly cooled to liquid air temperature is very extraordinary, a crystalline disc being almost exceptional.

Another simple method was to use a tambourine of a rubber membrane stretched on a light metal ring ($2\frac{1}{2}$ to 3 cm. diameter). This could be first carefully cooled, and the measured quantity of liquid (also previously cooled as low as was practicable) poured in and finally solidified by immersion. In some cases the rubber membrane could be stripped off under liquid air, leaving the thin solidified plate: but this is not necessary if the small absorption of the membrane is previously measured, and is very often not practicable, as, e.g., when cracks are formed during the cooling. Previous cooling of the membrane and liquid separately allows reactive or very solvent liquids to be readily handled without attack on the membrane or its support; but in many cases the tambourine was carefully cooled with the liquid already in. The tambourine was supported by a vertical wire bent to a springy horizontal ring, clamped above a cup of liquid air, which was steadily screwed up to cool the membrane. A deep quartz cup floating on the liquid air surface (but without much lateral freedom) was an advantage in some cases where more controlled and slower cooling was found necessary to give the best results.

Thus the ordinary condensable gases, such as ammonia, sulphurous acid, hydrochloric acid, etc., could usually be dripped from the end of a delivery tube just above the membrane tambourine, supported at the appropriate height above the liquid air. After the liquid had been solidified by further cooling, weighing of the tambourine on a scale pan in an open vessel immersed in liquid air presented no serious difficulties.* The thickness of the condensed material was

* Proc. Roy. Soc., A. lxxxix. p. 158.

therefore easily obtained. When necessary a correction was made for the quantity in the curved surface of contact between the liquid and the ring. Screens of approximately 0·1 mm. thickness were readily obtained by using a tambourine of silk-gauze chiffon instead of a rubber membrane. This was dipped into the liquid to be examined, and when lifted out retained a fairly uniform layer in the tiny spaces between the crossed silk threads. Separate measures of the proportionate obstruction of the stretched gauze alone showed that only 30 per cent. was stopped: a result found to be in agreement with microscopic measurements of the threads and spaces of the material. The wetted gauze was then cooled as rapidly as possible and weighed in the same way as the membranes, etc. The chief objection is that the mixed liquid and silk surfaces are not strictly plane.

The results obtained are grouped later, but some aspects of their interpretation should first be dealt with. An obvious correction to such measures is the reflection loss from the plane surfaces. This, as given by Fresnel, is $y = \left(\frac{n-1}{n+1}\right)^2$, where y is the proportion reflected at the surface, and n the index of refraction between the media separated by this surface.

The curve (Fig. 14) shows the growth of y (ordinate) with n the index of refraction (abscissa). The upper curve is for substances in air, and the lower curve gives the reduced values of y in liquid air for values of n, corrected by $\frac{1 \cdot 0}{1 \cdot 22}$, the relative indices for air, gaseous and liquid. Table III. gives the values for four typical substances with increasing n, corresponding to normally incident "D" line radiation.

TABLE III.

Substance	Ref. Index n	Per Cent. Reflected	
		Air	Liquid Air
Indiarubber . . .	1·5	4·0	1·1
Silver chloride . .	2·06	12·0	6·4
Iodine . . .	3·34	29·2	21·2
Stibnite . . .	5·29	45·2	38·8

This correction for the rubber membrane or rock-salt is within the experimental error, but for silver chloride is quite considerable. When this substance is used to obtain very thin capillary films of different liquids, the results may be quite misleading unless carefully analysed. So also for direct measurements of iodine or other very refractive materials. This is, however, fairly simple on the

assumption of ordinary "D" light, but the actual radiation measured is very different from this, and the consequent correction to be applied is doubtful.

In this connection it is as well to recall the altering wave length of the region of maximum radiation as the temperature of the source is changed. This is illustrated by Figs. 15 and 16. The first set of

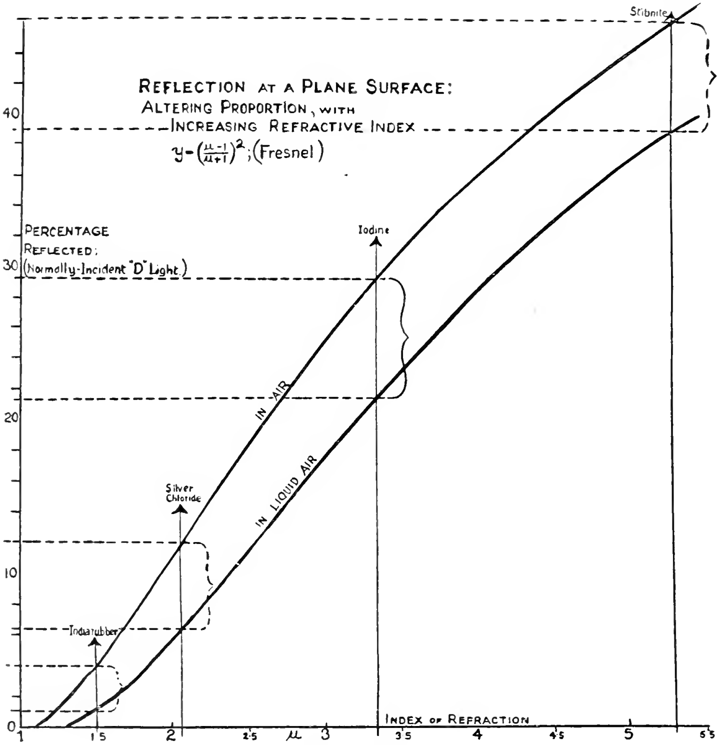

Fig. 14.

curves are from Lummer and Pringsheim's results. The second, at lower temperatures, are calculated from Planck's formula—

$$J_\lambda = \lambda^{-5} \times C_1 \left(e^{\frac{C_2}{\lambda T}} - 1\right)^{-1}$$

(as given in the Smithsonian Physical Tables, 1920, p. 247).

At a temperature of 1650° Abs. the maximum theoretical wave length emitted is of the order 2 μ, while by the time a source is

reached whose temperature is down to − 80° C., the maximum has increased to a wave length of 16 μ, and of course at lower tempera-

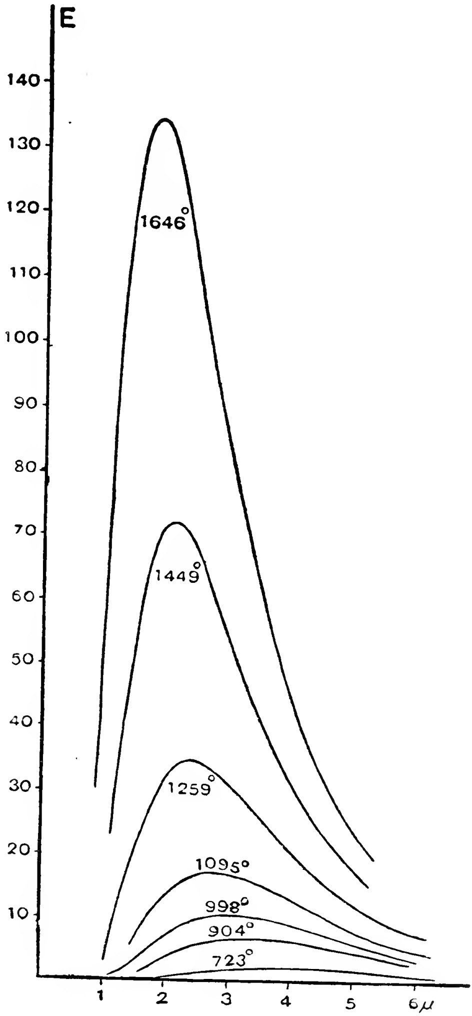

Fig. 15.

tures there would be even longer waves, a region scarcely yet explored with this thermoscope. Wien gives a simple relation

between wave length of maximum radiation intensity from black body and absolute temperature—viz. $\lambda T = 2930$, where λ is the wave length in microns (μ) and T is the absolute temperature. Some

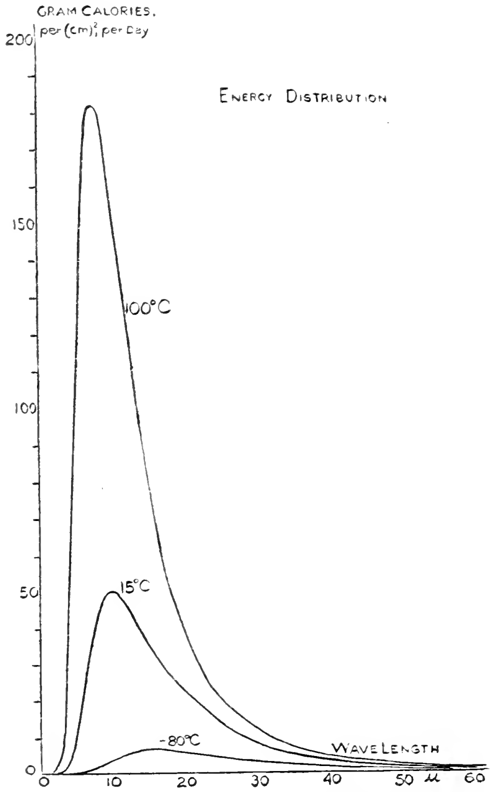

FIG. 16.—HEAT SPECTRUM AT THREE DIFFERENT TEMPERATURES.

typical temperatures and wave lengths calculated from this formula are given in Table IV.

TABLE IV.

T . .	373	273	195	90	78	50	20	14	5	2
λ . .	8	11	15	32	38	58	146	209	586	1465

Rubens and Kurlbaum, on the other hand, experimenting with specially sifted radiations of wave length, 24 μ and 31·6 μ, and using a black source down to liquid air temperatures, got results which do not agree with these values.

Assuming "D" line radiation, the following examples will illustrate the order of the corrections to be made. In the case of rock-salt there are only small variations of the index of refraction with rather large changes in wave length, as shown in Table V.

TABLE V.

Wave length (μ) .	0·6 ("D")	8	11	16
Refractive index .	1·545	1·507	1·481	1·44
T° Abs. . . .	—	373°	273°	183°

In this case, therefore, the error from taking the indices corresponding to the "D" line will not be very serious, a result which does not hold for silver chloride.

Taking the cases of films of water and alcohol respectively, between plates of silver chloride, the transmissions recorded were 18 per cent. for water and 58 per cent. for alcohol, the thickness of film being 0·038 mm. for water and 0·040 mm. for alcohol.

Now if for simplicity we take

$$R + A + T = 100 \text{ per cent.},$$

where R = the reflections, A = the absorptions, and T = the observed transmission, then of the reflections there will be two between silver chloride and liquid air surfaces, and two between silver chloride and ice surfaces. The calculated "Fresnel" reflection-corrections are 6·7 per cent. for the liquid air surfaces and 4·6 per cent. for the ice surfaces. The total reflection-corrections are therefore

$$2 \times 6{\cdot}7 + 2 \times 4{\cdot}6 = 22{\cdot}6 \text{ per cent.}$$

The absorption of the silver chloride plates (separately measured and corrected for liquid air reflections) is only 1·2 per cent., so that each plate reduces the incident radiation by 11·9 per cent.

Thus 88·1 per cent. is transmitted by each plate. Since 18 per cent. emerges from the second plate, the proportion transmitted by the ice between the plates is

$$18\left(\frac{100}{88{\cdot}1}\right)^2 = 23{\cdot}2 \text{ per cent.}$$

Similarly with the alcohol: assuming the same refractive index as for ice, the transmission by each plate is 88·1 per cent.: then since 54 per cent. emerges in this case, the proportion transmitted by the alcohol alone is

$$54\left(\frac{100}{88\cdot 1}\right)^2 = 69\cdot 6 \text{ per cent.}$$

Several other liquids were similarly studied, such as methylene iodide and chinoline. (See Fig. 17.)

A chinoline film gave observed transmission of 54 per cent. With a refractive index at each silver chloride-chinoline surface of 1·268, the reflected proportion will be 1·4 per cent.; this with the 6·7 per cent. at each silver chloride-liquid air surface would result

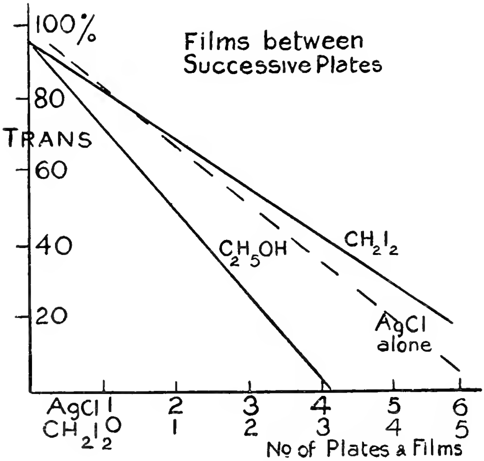

Fig. 17.

in a corrected transmission of 65 per cent. for the chinoline film alone. By the same method of taking a fractional diminution at successive surfaces, based on the refractive index through each surface, the transmissions were worked out for three films of alcohol and four films of methylene iodide, enclosed respectively by four and five silver chloride plates in succession. The transmissions were observed at each step as the piles of plates and solidified films were built up. The results are given in Fig. 17. The calculated transmissions through the successive alcohol films were 72·7 per cent., 65·2 per cent., and 65·1 per cent.; and through the methylene iodide films, 76·5 per cent., 79·1 per cent., 80·6 per cent., and 78·9 per cent. These values were deduced on a transmission factor of 0·9345 for

each silver chloride-alcohol surface, and 0·993 for each silver chloride-methylene iodide surface, applied to the observations obtained at each step.

Rock-salt with its lower refractive index would be preferable to silver chloride for such measurements, except that it has to be cooled so slowly to avoid cracking that much more time is required, whereas silver chloride in thin rolled plates is very easily handled. Two superposed membranes with liquid films between are also quite workable.

Successful trials were made with a hollowed-out flat capsule of rock-salt fitted with a plane cover plate when necessary. By this means plane layers of substances of definite thickness were obtained.

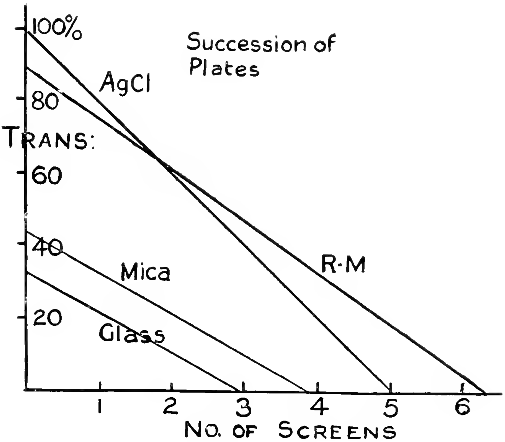

Fig. 18.

With ordinary liquids the whole reflection correction is very small, being practically the same as for a rock-salt plate alone—namely, 2·7 per cent. in liquid air (for the "D" line). This cell was to be used for measuring the transmissive powers of liquid gases (such as methane) with a "gas-cooled" thermoscope. With liquid immersion in the ordinary way, the cover plate would be necessary. This, however, was not carried out.

The transmission through a succession of plates of silver chloride alone was also measured for the purpose of studying the rate of extinction of incident radiation by successive reflections. Thin membranes and thin discs of mica and glass were also measured. The results observed are shown in Fig. 18.

A simplified method for several surfaces has been pointed out

by Provostaye and Desains.* Thus, if there are m surfaces, and $y = \left(\frac{n-1}{n+1}\right)^2$ reflected at each surface, then for m surfaces $Y = \frac{m\,y}{1+(m-1)\,y}$ will be reflected, and therefore $1 - Y = \frac{1-y}{1+(m-1)\,y}$ transmitted. This expression applied to (1) rock-salt, (2) silver chloride, and (3) iodine, gives the values shown in Table VI.

TABLE VI.

(1) *Rock-salt* (or glass, mica, rubber membrane, approximately).

		In air, $n = 1 \cdot 544$, $y = 0 \cdot 0456$.		*In liquid air, $n = 1 \cdot 266$, $y = 0 \cdot 0136$.*	
Plates	Surfaces	Y	1 − Y	Y	1 − Y
		per cent.	per cent.	per cent.	per cent.
1	2	8·7	91·3	2·68	97·32
2	4	16·04	83·96	5·23	94·77
3	6	22·3	77·7	7·64	92·36

(2) *Silver chloride.*

		In air, $n = 2 \cdot 06$, $y = 0 \cdot 12$.		*In liquid air, $n = 1 \cdot 69$, $y = 0 \cdot 0658$.*	
1	2	21·4	78·6	12·3	87·7
2	4	35·3	64·7	22·0	78·0
3	6	45·0	55·0	29·7	70·3

(3) *Iodine.*

		In air, $n = 3 \cdot 34$, $y = 0 \cdot 2907$.		*In liquid air, $n = 2 \cdot 738$, $y = 0 \cdot 2162$.*	
1	2	45·04	54·96	35·6	64·4
2	4	62·12	37·88	52·5	47·5
3	6	71·1	28·9	62·3	37·7

In the case of both silver chloride and rubber membranes, as shown by Fig. 18, the observed values diminished more rapidly than these tables indicate. Thus three silver chloride plates only transmitted 49 per cent. observed as against 70·3 per cent. if there were only reflection losses. When all account is taken of the indi-

* Ann. de Chim., xxx. p. 159, 1850.

vidual absorptions, there still appears some discrepancy, so that probably the index of refraction employed (that for "D" light) is not sufficiently near for the dark radiation employed, especially with a substance of such relatively high index as silver chloride. The observed values may, however, be utilised in a reverse calculation to that given above—i.e. to determine the effective index of refraction to give the observed reflection losses. Using the observed $Y = 0{\cdot}51$ (49 per cent. transmitted) and $m = 6$, then $y = 0{\cdot}148$, whence $n = 2{\cdot}25$, instead of the $1{\cdot}69$ employed in the previous table.

Tabulated Values of Transmissive Power.

Owing to the great heat-absorptive power of water, the principal precaution to be taken in preparing substances for these observations is to ensure perfect dehydration.

Among the more simple bodies carbon disulphide and iodine are well known as very perfect transmitters of heat at ordinary temperatures, and they preserve this property at low temperatures. Tetrachloride of carbon and thiophosgene are similarly very transmissive. Phosphorus is remarkable, for while its observed transmission is 76 per cent., when this is corrected for reflections less than 10 per cent. remains to represent the maximum absorption, which is thus even less than that of iodine, and of the same order as rock-salt and the chloride and bromide of silver.

The paraffins and benzene exhibit high transmissive powers, and halogen substitution generally increases the value. Alcohols, ketones, acids and similar bodies are absorptive, as also are amides, nitro-bodies, etc. Esters are generally more transmissive, while pyridine and its associates are still better.

The principal results are grouped in the following tables. There are four columns in addition to the one describing the substance. Of these the first indicates the condition of the cooled substance: G being for a glass and C for a crystalline structure. The support or means of securing the disc is shown in the second column: RM indicates that the material was solidified in a rubber-membrane tambourine, Ch that the chiffon gauze was employed as already described, and P that the material was in the form of a plate compressed or frozen. The thickness in mm. is given in the third column, and the observed per cent. transmission in the fourth.

Substance	1	2	3	4
Simple Compounds, etc.				
Sulphuric acid	G	P	0·6	2·5
Hydrochloric acid	G	RM	0·5	11
Sulphurous acid	C	,,	,,	20
Ammonia	C	,,	2·0	4
Phosphorus *	—	P	0·75	90
Iodine *	—	,,	0·8	80
Bromine	G	,,	0·5	10
Carbon tetrachloride	C	RM	,,	56
Thiophosgene	G	Ch	0·1	53
Carbon disulphide	G	RM	0·5	70
Mercaptan	G	RM	0·5	21
Sodium	—	—	1·0	4
	—	—	0·1	8
Selenite	—	—	1·0	6
Chrome alum	—	—	3·0	4

* Corrected for reflection.

Common Substances				
Glass	—	—	0·25	14
	—	—	0·125	19
	—	—	0·012	37
	—	—	0·006	40
Mica, white	—	—	0·1	5
	—	—	0·063	8
	—	—	0·025	24
	—	—	0·013	33
,, black	—	—	0·17	10
	—	—	0·06	13
	—	—	0·023	26
	—	—	0·013	33
Celluloid	—	—	0·125	7
Collodion films	—	—	Blue†	86
	—	—	Amber†	90
	—	—	Black†	94
Gelatin	—	—	0·02	35
,, 2 discs	—	—	2 × ,,	15
,, 3 ,, ...	—	—	3 × ,,	10
Goldbeater's skin	—	—	—	15
Ebonite	—	—	0·5	13
India-rubber	—	—	0·625	4
	—	—	0·25	9
	—	—	0·1	30
	—	—	0·05	55
	—	—	0·02	80

† By reflected light.

Substance	1	2	3	4
Hydrocarbons (Aliphatic)				
Pentane (n. or i.)	G	RM	0·5	53
,, iso-propyl ethane	G	RM	0·5	74
	,,	Ch	0·1	77
Hexane n.	C	RM	0·5	47
	C	Ch	0·1	55
Heptane	G	RM	0·5	51
Octane	G	RM	0·5	60
Decane (di-iso-amyl)	G	RM	3·1	32
	G	,,	0·5	52
	G	Ch	0·1	78
Amylene	G	RM	0·5	15
Diallyl	G	RM	0·5	21
Dipropargyl	G	RM	0·5	11
Isoprene	G	Ch	0·1	40
β γ Dimethyl butadiene	G	Ch	0·1	74

Hydrocarbons (Aromatic)				
Benzene	G	RM	0·5	49
	,,	Ch	0·1	55
(Dipropargyl	,,	RM	0·5	11)
Toluene	,,	,,	,,	34
Ethyl-benzene	,,	,,	,,	28
Propyl- ,, ...	,,	,,	,,	27
Xylene	,,	,,	,,	25
Cyclo-hexane	,,	,,	,,	38
	,,	Ch	0·1	53
(Cyclo-hexanol	,,	RM	0·5	19)
(Cyclo-hexanone	,,	,,	,,	20)
Naphthalene	C	P	1·3	19

Halogen substituted bodies				
Methyl iodide	G	RM	0·5	40
Ethyl chloride	,,	,,	,,	36
,, bromide	,,	,,	,,	40
,, iodide	,,	,,	,,	53
Propyl iodide (n.)	,,	,,	,,	24
Allyl ,, ...	,,	,,	,,	44
Hexyl ,, ...	,,	,,	,,	28
Chloroform	C	,,	,,	43
	C	Ch	0·1	53
Methylene chloride	G	RM	0·5	51
,, iodide	,,	,,	,,	45
Ethylidene chloride	,,	,,	,,	40
Acetylene tetrachloride	,,	,,	,,	53

Substance	1	2	3	4
Halogen substituted bodies (cont.)				
Chlor-benzene	G	RM	0·5	39
Di- ,, ,, ...	,,	,,	,,	59
Per-,, ,, ...	—	P	1·6	10
Benzene hexachloride	—	P	1·0	13
Brom-benzene	G	RM	0·5	31
Iodo- ,, ...	C	,,	,,	24
Pyridine dichloride	,,	P	,,	37
,, tetra ,, ...	,,	RM	,,	35
,, penta ,, ...	,,	,,	,,	40
α Chlor-naphthalene	G	,,	,,	42
α Brom- ,, ...	,,	,,	,,	42
(Glycerol	,,	,,	,,	5)
Mono-chlor-hydrin	,,	,,	,,	9
Di- ,, ,, ...	,,	,,	,,	13
Tri- ,, ,, ...	,,	,,	,,	17
Epi- ,, ,, ...	,,	,,	,,	7

Alcohols, Aldehydes, Ketones and Ethers				
Methyl alcohol	G	RM	0·5	18
	,,	Ch	0·1	27
Ethyl alcohol	,,	P	1·5	8
	,,	RM	0·5	17
	,,	Ch	0·1	38
	,,	—	0·04	70
Propyl alcohol	,,	RM	0·5	20
	,,	Ch	0·1	22
Propyl alcohol, iso	,,	RM	0·5	15
Butyl alcohol n.	,,	RM	0·5	33
,, ,, tertiary	,,	Ch	0·1	57
Amyl alcohol ,, ...	,,	RM	0·5	36
	,,	Ch	0·1	41
Ethylene alcohol	,,	RM	0·5	10
Propylene ,, ...	,,	,,	,,	8
Allyl ,, ...	,,	,,	,,	20
Paraldehyde	C	,,	2·1	27
	C	,,	0·5	38
Chloral	G	,,	,,	25
Propylaldehyde	,,	,,	,,	8
Acetone	C	,,	,,	25
Methyl-ethyl-ketone	C	,,	,,	19
Ether	G	,,	1·5	37
	,,	,,	0·5	41
	,,	Ch	0·1	46
Glycerol	,,	RM	0·5	5
Phenol	C	Ch	0·1	32
Benzaldehyde	G	RM	0·5	28
(Benzoic acid	—	P	1·7	9)
Salicylaldehyde	C	RM	0·5	10·5
Acetophenone	C	,,	,,	16

Substance	1	2	3	4
Acids and Anhydrides, etc.				
Formic acid	C	RM	0·5	8
Acetic ,, ...	G	,,	,,	7
Monochloracetic acid	G	,,	,,	10
Di- ,, ,, ...	,,	,,	,,	16
Tri- ,, ,, ...	,,	,,	,,	19
Proprionic acid	C	,,	,,	5
Valeric acid (sec)	G	,,	,,	2·5
Butyric ,, n.	G	,,	,,	4
(α Br ,, ,, ...	,,	,,	,,	5)
Stearic ,, ...	C	,,	,,	21
Oleic ,, ...	G	Ch	0·1	24
Acetic anhydride	G	RM	0·5	20
Acetyl chloride	G	,,	,,	20
Esters				
Methyl acetate	C	RM	0·5	25
Ethyl formate	G	,,	,,	16
,, acetate	,,	,,	0·25	20
	,,	Ch	0·1	46
,, chlor acetate	,,	RM	0·5	28
Aceto-acetic ether	,,	,,	,,	5
Ethylene acetate	,,	,,	,,	15
Amyl ,, ...	,,	,,	,,	14
Methyl oxalate	C	,,	,,	8
	,,	Ch	0·1	20
Ethyl ,, ...	,,	RM	0·5	15
Ethyl carbonate	G	,,	,,	24
Ethyl ester of eth. malonic acid ...	C	,,	,,	15
,, ,, ,, me. ,, ,, ...	C	,,	,,	8
Methyl ,, ,, malonic acid	G	,,	,,	27
Glycol bi-acetate	,,	,,	,,	4
Triacetin	,,	,,	,,	25
Amines, Amides, etc.				
Trimethylamine...	G	RM	0·5	13
Triethylamine	,,	,,	,,	24
Amylamine	,,	,,	,,	10
Formamide	C	,,	,,	1[illegible]
Acetamide	G	,,	,,	27
Urea	G	Ch	0·1	11
Aniline	G	RM	1·4	21
Diethyl aniline	,,	,,	0·5	28
Dimethyl ,, ...	C	,,	,,	28
Toluidene o.	G	,,	,,	11
Xylidene 1 : 3 : 4	,,	,,	,,	14
Phenylhydrazine	,,	,,	,,	2

Substance	1	2	3	4
Amines, amides, etc. (cont.)				
NH_4I	—	P	0·7	16
$N(CH_3)_4I$	—	,,	1·5	43
$N(C_2H_5)_4I$	—	,,	1·1	22
Cyanides, Nitriles, Nitro-bodies, etc.				
Cyanogen...	G	RM	0·5	30
Aceto-nitrile	C	,,	,,	28
Proprio- ,, ...	,,	,,	,,	18
Valero- ,, (iso)	G	,,	,,	19
Butyro- ,, ...	,,	,,	,,	15
Ethyl cyanoacetate	,,	,,	,,	8
Amyl nitrite	,,	,,	,,	6½
Nitromethane	G	,,	,,	35
Benzyl cyanide	G	,,	,,	12
Benzonitrile	C	,,	,,	19
Nitrobenzene	C	,,	,,	21
,, toluene	C	,,	,,	10
Pyridine and allied bodies				
Pyridine	G	RM	0·5	42
Picoline	,,	,,	,,	53
Lutidene	,,	,,	,,	35
Chinoline...	,,	P	0·7	30
Pyrrol	,,	Ch	0·1	28
Nicotine	,,	RM	0·5	10
(Thiophene	,,	,,	,,	28)
(Furfural	,,	,,	,,	18)
Tri-azo bodies				
Tri-azo benzene	G	RM	0·5	24
,, ethanol	,,	,,	,,	2½
,, acetic ester	,,	,,	,,	16
,, ethyl acetate	,,	,,	,,	8
Ethyl tri-azo acetate	,,	,,	,,	6
,, ,, proprionate	,,	,,	,,	13
Turpentine	G	RM	1·0	2
	,,	,,	0·5	5
Camphor	—	P	1·3	10
Monobrom-camphor	—	,,	1·5	21
α Dibrom- ,, ...	—	,,	1·3	6

Several normally absorptive substances when sufficiently reduced in thickness were found to behave like india-rubber and permit a considerable transmission. The curves of Fig. 19 give some instances.

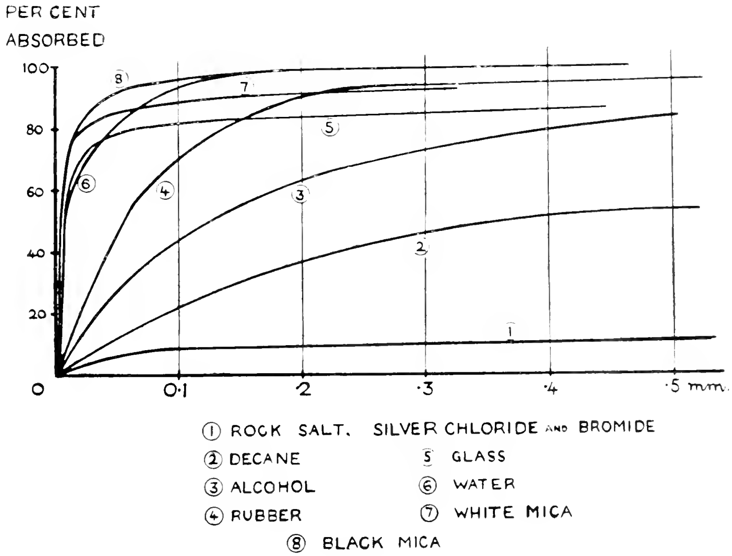

Fig. 19.

In the case of bodies crystalline at low temperatures some of the heat will be diffused. To estimate the possible error thus involved several preparations were made of rock-salt, graded from polished transparent plates to opaque blocks. The resulting values are shown in the next table.

Rock-Salt Transmissions.

Rock-Salt, 1 mm. thick	Heat Transmission
Plate, 2 polished surfaces	90% to 94%
,, 1 ,, and 1 rough surface ...	72% to 76%
,, 2 roughened surfaces	62% to 64%
Ground finely and compressed	70%
Ditto ditto after brine moistening ...	31%
NaCl precipitated by alcohol; dried and compressed	50%
Ditto ditto, after brine moistening ...	18%

The comparison between polished rolled silver chloride and bromide and a compressed plate of silver iodide is also interesting,

and is given in the following table (without any correction for reflection) :—

Plate	Thickness	Transmitted proportion
Silver chloride	0·15 to 1 mm.	84% to 85%
Silver bromide	0·5 to 1 mm.	83% to 86%
Silver iodide...	1·2 mm.	65%

A certain number of observations were made with the black body source cooled by boiling solid carbonic acid. The values obtained were not very different from those already given. The following table gives comparative results for a few typical bodies :—

Substance	Temperature of Source			
	−78° C	11° C	100° C	
Rock salt	91	94	94	Transmission
Silver chloride ...	82	80	80	
Rubber membrane ...	78	76	75	
Mica (0·025 mm.) ...	33	30	28	
Glass (0·125 mm.) ...	21	20	19	

Observations of the emissivity of various substances, as polished plates, placed above the thermoscope gave the following values in comparison with the black Leslie cube :—

EMISSIVITIES AT 100° C.

Thermoscope Observations.

"Black Body"	100
Iron	14
Lead	14
Tin	12
Aluminium	9
Zinc	7
Nickel Silver	6
Pewter	5
Copper	5
Brass	5
Mercury	3

Acknowledgment is due to Mr. W. J. Green, B.Sc., for the valuable assistance he rendered during the progress of the work and in the preparation of this abstract.

[J. D.]

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ALBEMARLE STREET, LONDON, W.1

February 1924

WEEKLY EVENING MEETING,

Friday, February 4, 1921.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

A. D. WALLER, M.D. LL.D. D.Sc. F.R.S.

The Electrical Expression of Human Emotion.

WE are all of us familiar, subjectively within ourselves, objectively by the behaviour of our neighbours, with the signs and symptoms of emotion, and with the fact that such signs and symptoms are more or less under voluntary control and can be suppressed or simulated at will. We are moved to or from an object we may desire or fear. We are moved to laughter or to tears by events witnessed and imagined; and whereas all men are moved in the mass by the same general motives of light and dark, food and hunger, love and hate, we know by everyday experience that no two men react in identical fashion to the same motives.

1. Physiologically, all emotions are expressed as neural outbursts from the central nervous system through efferent nerves to muscles and glands; emotion, in general, results in intensified physiological activity at the periphery of the body—muscles and glands, heart and blood-vessels, the face and eyes and skin. A movement of surprise, a palpitation of the heart, a blush, a pallor, a shiver, a rush of tears, a dilated pupil—all these and other signs of emotion consist in sudden local intensifications of the chemical exchanges that are in constant operation between the living cells of the body and the fluid medium by which they are surrounded. We know indeed that all such chemical exchanges are controlled through efferent nerves, and we speak of this control as their trophic action, but we are scarcely prepared at the present day to recognise the close association between signs of emotion and the phenomena of nutrition.

2. The physical sign of emotion is known to psychologists as the psycho-galvanic reflex. It was first definitely revealed to us twelve years ago by Veraguth,* of Zurich, and has since then formed a favourite subject of study by many later observers whom I shall not attempt to enumerate. I joined in the hunt four years ago,† and

* "Das Psychogalvanische Reflexphenomen." (Berlin, 1900.)

† "The Galvanometric Measurement of 'Emotive' Physiological Changes." Proceedings of the Royal Society, B, vol. xc., p. 214, 1917.

was very quickly satisfied that this physical sign affords the most convenient possible gauge and measure of human character and of human temperament, seeing that it declares *how much* a given subject is moved by his thoughts and feelings. A spot of light showing the movements of a galvanometer connected with the palm of the hand exhibits the fluctuating emotions of the person to whom the hand belongs and if the person be an ordinary normal person it is only the palm of the hand, and not any other part of the skin of the upper extremity, that shows the response. My first point is, then, that the emotive response is, *par excellence*, a palmar phenomenon, and I shall, as my first and chief experiment, undertake to demonstrate this point. [Experiment.]

3. Mr. X. Y. has been good enough to lend himself to my purpose. His hand and his forearm are connected with each of galvanometers and two Wheatstone bridges. The round spot belongs to the hand circuit, the square spot to the forearm circuit, and balance can be adjusted in each circuit separately by suitable manipulation of the two resistance boxes. In both cases the wiring is such that increased conductivity of the hand or of the forearm gives movement of the spots to my right—i.e. any emotive impulses from the brain down motor nerves to the hand or to the forearm will cause deflection to the right. Let us watch the two spots for a while. I expect you to see that the hand spot behaves irregularly, whereas the arm spot creeps steadily across the scale without showing any of the vagaries of the round spot.

You realise now why I have been at trouble to show the simultaneous behaviour of *two* spots. With only the hand in circuit of one galvanometer you should at first have felt doubtful whether the movements you saw were really due to emotive discharges, and not to otherwise imperceptible muscular twitchings such as are perceived and utilised by thought-readers. It would otherwise have been desirable to set up some very delicate form of myograph to satisfy this doubt. I shall show you presently, by asking the subject to make a least possible movement of one of his fingers, that the round spot—i.e. that indicating the electrical resistance of the hand—shows a deflection which is due to a minute disturbance of contact, and, therefore, takes place in the direction opposed to that of an emotive response. I am sure you will realise with me what a mercy it is that the deflection by slight, often quite unavoidable movement is, in general. the contrary of that of the emotive response.

4. But to return to our experiment. The subject is at rest; both spots are reasonably steady, but by reason of his past experience he knows that an evil moment is approaching. As you may see by the irregular movements of the hand spot, he is beginning to worry, making a picture in his mind of the pain he is about to undergo by steel or fire, and, obviously, this disturbance of quietude creates a condition that is not favourable for recognising or measuring the

disturbing effect of any real interference with his comfort. The emotive effects of my threatening language must be allowed to subside. You cannot expect to study rings made by throwing a stone into a pond unless the pond is quiet; you must wait for it to get still. When he comes to rest Mr. X. Y. will react smartly and obviously in response to the suddenly threatened pin-prick or to a real pin-prick. [Trials by pin and matches. Real and imaginary pin-pricks and burns.]

You now, perhaps, feel fairly well satisfied that a statement made a few minutes ago is correct. In the upper limb of a normal person emotive responses to slight excitations are confined to the palm of the hand. The only other part of the body in which they occur is the sole of the foot, but this I shall ask you to take on trust; it really is not necessary that the actual evidence should be brought into court. It would merely be a repetition of what you have just witnessed; and this lantern-plate (Fig. 1) will, after all, afford us the quickest, as well as the most conclusive, evidence.

5. I shall venture to trespass just a little further upon Mr. X. Y.'s endurance to make good one further point, although it is a point that you may already have noticed.

This palmar emotive response is, in my view, to be regarded as caused by a sudden augmentation of electrical conductivity in a membrane or membranes in the fourth arm of the Wheatstone square. That augmentation of conductivity is to be understood as produced by a sudden dilatation of ultramicroscopic pores in this membrane or membranes. I am not speaking of visible pores, but of invisible pores such as are postulated in theories of electrical conduction and of osmotic phenomena. I imagine that these invisible pores suddenly dilate when the emotive impulse through efferent nerves reaches the living membrane, just as we see the pupil of the eye dilate with an emotion of surprise. And with this image in my mind I find it extremely interesting to recognise and measure what a very long time it takes for any given stimulus to produce its effect. It takes two seconds before the threat of a pin-prick—or, for the matter of that, an actual pin-prick—or a single induction shock, brings about the sudden dilatation of pores or increased permeability and the increased electrical conductivity that are signified to us by the movement of the spot of light. How is this long lag of two seconds to be accounted for? Does it occur on the afferent side? Assuredly not. A delay of this sort might be expected to amount to at most one-fifth of a second. Moreover, if we miss out the afferent side altogether, and bring about the response by an artificial explosion down efferent nerves, we shall find the same long delay of two seconds between the muscular movement and the emotive movement, both of which are taking place at the periphery. Therefore, the chief business of the long delay takes place at the periphery, in the skin of the palm of the hand, and its great length is a token that

we have to do with impulses conveyed, not along cerebrospinal, but along sympathetic nerves. We may find time later to discuss the question whether these are vasomotor or secretomotor or trophic nerves.

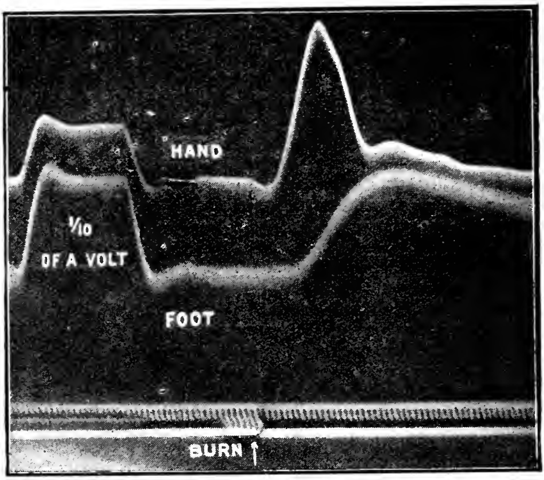

FIG. 1.—This photogram is the simultaneous response of the HAND and of the FOOT of a normal subject, and is given as an example to illustrate the method of investigation by double response. In this example it is evident: (1) that the response occurs sooner and is of shorter duration in the hand (palm) than in the foot (sole). From the time record it can be seen that the lost times in the hand and in the foot are respectively 2 and 4 seconds (approx.), and the durations of response 15 and 40 seconds (approx.). A closer approximation to true time values would require a quicker record to be taken for the lost times and a slower record for the durations. (2) That the response is greater in the hand than in the foot. This magnitude is measured by reference to the initial deflections made by passing a current from $\frac{1}{10}$ volt through each of the two circuits. In this example the hand response is approximately $\frac{1}{10}$, and the foot response approximately $\frac{2}{10}$. The rate of movement of the plate is shown (not very distinctly) in half-seconds. The portion shown in the figure occupied about 45 seconds. A similar procedure by simultaneous double response is required for the mapping out of the body-surface. Obviously the comparison between right and left sides, upper and lower extremities, distal and proximal parts, flexor and extensor aspects, is to be carried out with far greater expedition and certainty by double than it could be by single records.

6. *Dreams* are subjective phenomena occurring in the subconscious state, with which we are all familiar during sleep, and during the

hypnotic state, and in the state called "trance." We are familiar also with innumerable objective signs of such subjective phenomena in the shape of descriptions of dreams and in the behaviour of sleep-talkers and sleep-walkers, and, above all, in the extraordinary cases of spiritualistic mediums. These last stand highest in the scale of sensitiveness.

The relative magnitudes of response to real pin-prick and to a fictitious pin-prick vary with different people under different conditions, but in general they may be divided into two categories, whom we may call *positives* and *imaginatives*.

Positives—in whom little or no disturbance is caused by the threat of a pin-prick, and a real pin-prick is required before any response takes place.

Imaginatives—in whom a large response occurs to the threat—larger, it may be, than the response to the real fact. In not a few of this imaginative class it is almost impossible to take a pure observation of response to fact, for they begin to respond as soon as the operator makes the slightest movement, or else the response is a large one, compounded of fear followed by fact. Here is a confirmatory experiment in evidence of what may be characterised as a dwindling fear and its revival by fact. [Experiment.]

All men (and, judged by their behaviour, animals also) are more or less imaginative. The kind of diagram you have just seen would represent the responses of nine out of ten of my present hearers to a series of threats with a real shock interpolated in the series. Many of us had an opportunity a few years ago of studying upon our friends and upon ourselves the signs and symptoms of fear during German air raids upon what they called the fortified city of London. The noise and disturbance occasioned by these raids, the false alarms and the warnings by maroons and sirens, afforded a unique opportunity for the exact galvanometric study of the emotions aroused by various kinds of noises. From the purely scientific point of view the opportunity could not be neglected of studying the psychophysical phenomena brought to our doors—phenomena that could not be expected again within the same lifetime. So from the air raid of September 21, 1917, to the last and most prolonged visit of Whitsuntide, 1918, I enlisted the services of volunteers to sit quietly, connected by wires to a galvanometer, and on two occasions I had sitters arranged in connection with recording apparatus which was set going a few minutes before the noise began, so that the emotive response during the whole affair was recorded. Let me show you two or three photographs (Figs. 2 and 3).

These photographs are not merely of interest on their human side, but also have this definite scientific value, that they afford measured records of the largest emotive responses that I have ever witnessed. The responses commonly observed in the laboratory are at most 10 per cent. changes; these air-raid responses have been at least

200 per cent. changes, which I cannot reproduce artificially by any means I care to employ.

7. But to return to our different classes according to sensitiveness. We classified people as positives and imaginatives according as they exhibited greater response to fact or to fiction. Apart from this criterion, we might undertake to arrange people as more or less imaginative according as they give larger or smaller responses to certain standard threats, as of a pin-prick or the lighting of a match. High in the scale of imaginatives we not infrequently meet with people who can at will either keep quiet, or think thoughts and see

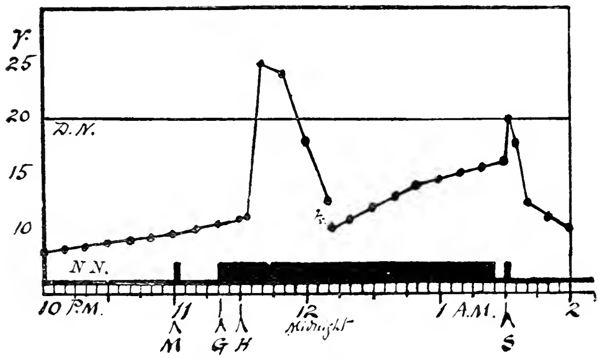

Fig. 2.—Emotivity of A. M. W. during the air-raid on Whitsunday, 1918. (From the *Lancet.*) M indicates the time of the first warning by maroons at 11 p.m. G indicates the commencement of gun-fire. The duration of the disturbance was from 11.20 p.m. to 1.30 a.m. H marks the moment of maximum alarm, when the swelling hum of approaching aeroplanes was most audible. S indicates the second warning by siren at the termination of the disturbance. The electrodes were transferred from the left to the right hand at 12.5. The horizontal lines D.N.–N.N. indicate the average normal day and night conductance of A. M. W., ascertained from other observations.

visions and hear words of purely imaginary existence without objective physical substratum. It is very interesting to watch the galvanometric signs of subjective phenomena—interesting to the onlooker, but far more interesting to the subject who knows what he (or she) is thinking about. And when it is realised that the galvanometer answers to one's thoughts and temper, it becomes quite an absorbing pastime to sit quietly in an armchair and watch oneself think as one watches the galvanometer move.

8. The emotive response is liable to all manner of variations. It varies in different individuals, and in the same individual it varies with different states of mind and body. It varies in magnitude and

in its distribution over the limbs with variations in the magnitude of its exciting cause. While it is, in the main, an uncontrollable phenomenon, I call to mind more than one case where to all appearance it has been influenced at will.

9. The *distribution* of the response over the body is especially interesting. In normal persons it is exclusively palmar (and plantar); the rest of the body-surface is silent. But in "sensitives" it extends

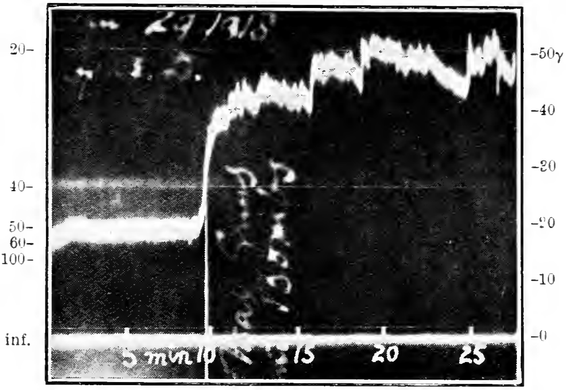

FIG. 3.—Galvanometric record of G. de D. during the air-raid of January 29, 1918. (From the *Lancet.*) At the tenth minute of observation the noise of maroons, immediately followed by that of aeroplanes and guns, broke out, and the resistance, which was approximately 60,000 ohms during the first ten minutes before the disturbance, fell to approximately 20,000 ohms during the next fifteen minutes. (On the left hand is given the resistance in thousands of ohms, and on the right the conductance in gemmhos.) The measurements are as follows:—

0	8.30 p.m.	56 × 1000	ohms	or	18γ
5	8.35 ,,	53	,,	,,	19γ
10	8.40 ,,	27	,,	,,	27γ
15	8.45 ,,	22	,,	,,	45γ
20	8.50 ,,	20	,,	,,	50γ
25	8.55 ,,	20	,,	,,	50γ
At 11.30 (all quiet)		44	,,	,,	23γ

up the limbs and the trunk. And a border-land person, according to his state of temper, can react normally to-day, but as a "sensitive" to-morrow. The few spiritualistic mediums whom I have examined have (with one doubtful exception) given the reaction proper to "sensitives," i.e. in the hand and in the forearm.

10. The *diurnal variations* of the reaction attracted my attention at the very outset of the inquiry. I soon noticed that the same

people, when submitted to a standard stimulus at different times of the day, gave responses of very different magnitudes; the responses were at their best about the middle hours of the day, when physiological activity is high, as compared with what was elicited early in the morning and late at night. And the conductivity of the palm of the hand rose and fell during the day (as does the temperature).

I thought it necessary to investigate this diurnal periodicity rather closely to learn how much it might be necessary to take into account the time of day when comparing results obtained on different individuals. So I watched this periodicity on my own hands by means of apparatus set up for the purpose in my dressing-room, so that observations of conductivity could be taken at any convenient time. The observations were recorded to form a graph on squared paper; and it may be remarked, by the way, that throughout the observations the conductivity of my right hand has been found to be higher than that of my left hand.

11. The three weeks over which these observations extended afforded me an admirable opportunity of observing the galvanometric effects of my own normal variations of "temper." Most people are more or less conscious of what may perhaps be called variations of euphoria before breakfast, and of very distinct, if not outwardly evident, variations of euphoria when the morning's letters are read. In order to test this point a photographic recorder was set up in connection with the galvanometer on my dressing-table, and I had my letters brought up there and read to me and signalled on the recording plate. Most of the letters made no impression upon me, but I well remember one fortunate morning on which the post included two distinctly effective letters which produced marked effects duly recorded on the photographic plate.

12. One is naturally tempted to ask what relation there may be between the magnitude of the reaction and the mental quality. A first step towards an answer to this question has been taken by Miss Waller, who has made systematic measurements of seventy-three students of medicine, divided according to examination results into an upper and a lower division. The average response was higher in the former than in the latter—e.g. to disturbing questions the average value of the response came out about 50 per cent. higher in the upper division.*

13. I have often been asked whether pleasant and painful sensations produce similar or opposed galvanometric deflections. The emotive response in its unmistakable form as a sharp movement

* Mary D. Waller, "The Emotive Response of a Class of Seventy-three Students of Medicine measured in Correlation with the Result of a Written Examination," *Lancet*, April 6, 1918.

occurring about two seconds after its exciting cause is always in one direction, i.e. in the direction of decreased resistance—increased permeability, poro-dilatation, or, if you prefer to think it so, contraction of living matter round pores so as to dilate them. And in many thousands of observations I have never witnessed any similar movement in the opposite direction—i.e. in the direction of increased resistance. All that is ever seen in that direction is the gradual remission of a previous deflection in the emotive or excitatory direction. If you regard the question in its psychological aspect, you will soon be satisfied that the matter could not be expected to come out otherwise. Our pleasures and pains are not simple opposites producing opposite physiological effects. Pains are active and exciting states in our conscious life, sharply contrasting with their background. Pleasure is more often merely the subsidence and relief from pain, a gradual recovery of the untroubled state. A pin-prick suddenly excites emotion, and the emotion gradually falls to rest. There is no counterpart pleasure equal and opposite to a pin-prick. Pleasure is of necessity gradual. Too sudden pleasure—joy, as we call it—is exciting, and causes discharge down the nerves that acts precisely like painful excitements and gives rise to electrical effects in the same emotive direction.

14. We distinguished a few moments ago between imaginatives and positives according as threatened pains produced larger or smaller effects than real pains. It is convenient to draw another kind of distinction according to the extent of body-surface over which the response is manifested. The response to "weak" stimuli in the great majority of men and women is exclusively palmar (and plantar). But with "strong" stimuli, and in certain cases with "weak" stimuli as well, the response can also be manifested by the forearm (and by the leg) as well as by the hand (and foot). Such cases may be designated as "sensitives" to distinguish them from the others who are relatively insensitive, but since these others are in a majority, and it would seem inappropriate to designate the majority of mankind as insensitive, it is better to call them "normals." These two labels, "sensitives" and "normals," are not intended to imply any division into two hard-and-fast categories, but rather a scale of differences grading between two extremes. Indeed, I have satisfied myself in at least one case that a subject classified at a first sitting as "normal" was temporarily raised to the degree of "sensitive" in consequence of a rather violent fit of "temper."

It is convenient to reserve the designation "insensitive" for cases low down in the normal scale, giving in response to ordinary stimuli little or no palmar reaction—i.e. a doubtful response of the order of 1 per 100 of the initial resistance.

15. Provisionally, then, our observations can be systematised in accordance with the following scheme :—

Class	Emotive Response		Examples
	Hand	Forearm	
I. Sensitives . . . ("Imaginatives")	Yes	Yes	Spiritualistic mediums and others
II. Normals . . .	Yes	No	The majority of men and women
III. Insensitives . . ("Positives")	No	No	Pythiatics. "Shell shock" cases
IV. Others . . .	—	—	"Shell shock" cases and others

Class I.—"Sensitives" giving large responses (10 per cent. or more of the original resistance) from the forearm and from the hand.

Class II.—"Normals" giving moderately large response (2 to 5 per cent.) from the hand, but little or no response from the forearm.

Class III.—"Insensitives" giving little or no response (1 per cent.) from the hand, and, of course, also the forearm.

Class IV.—(*a*) Subjects who, by reason of their state of health, were obviously unfit to undergo examination, and (*b*) subjects who declared themselves as unable to stand it.

Subjects of Class I. and Class II. include those who were characterised a moment ago as "imaginatives." The three spiritualistic mediums to whom I referred just now were included in Class I. Class III. comprises people of duller imagination, or perhaps of firmer fibre, whom we called "positives."

At this early stage indeed, when the number of properly observed cases is so small and the danger of imperfect observation so great, it seems to me hazardous even to talk about rules and exceptions or to attempt a classification. Nevertheless, if the attempt is made without prejudice, and if the results of observation are recorded in physical units by the side of what in medical parlance is the clinical history of the subject, a preliminary classification is not only permissible, but also necessary.

Let me again refer to the present attempt and make good the point that we may expect to find the unexpected, that so-called regular results may be exceptional and *vice versa*.

16. *Pythiatics.*—Hysterical subjects, or, as they are now called, "pythiatics," men as well as women, seem to be exceedingly sensitive and make a great fuss; but when they have been persuaded to sit still in an armchair and connected up with the galvanometer and tested by ordinary stimuli—pin-prick, false and real; match-burn,

false and real—lo and behold! they exhibit little or no response. They belong to Class III., that of the "insensitives"; and we are reminded of the fact that in exaggerated—i.e. pathological—degree the hysterical or pythiatic state is found to include anæsthesia, loss of sensibility, as a leading symptom. But, of course, more observations are necessary, and more observers.

[A. D. W.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

John McIlvaine Cater,
Clifford Copland Paterson, O.B.E.
Oswald Western,

were elected Members.

The Secretary reported the decease of Dr. Emile Ador on November 25, 1920, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to place on record their sense of the great loss sustained by the Institution by the death of Emile Ador.

Dr. Ador studied Chemistry in Edinburgh and London in 1865–1866, and the results of his important investigations on the Synthesis of the Ketones, Benzoic Acid and Benzophenone, and kindred subjects, are embodied in numerous communications to learned societies and scientific periodicals. In 1894 he contributed to the Archives des Sciences an important monograph on the life and work of J. C. Galissard de Marignac.

Dr. Ador was elected an Honorary Member of the Royal Institution on the occasion of its Centenary in 1899.

The Managers desire to convey to the family the expression of their deep sympathy with them in their bereavement.

The following letters from Honorary Members elected at the General Meeting of the Members on December 6, 1920, were read :—

[*Translations.*]

FACULTÉ DES SCIENCES DE MARSEILLE,
MARSEILLE,
December 14, 1920.

The President and Members of the Royal Institution of Great Britain.

I have been informed of the Vote by which you have nominated me an Honorary Member of the Royal Institution. Allow me to express my recognition of the great honour you have conferred upon me.

My deep admiration for the Science of your country, and in particular for English Physics, which continue so brilliantly a venerable tradition, the glorious rôle which the Royal Institution has played in the development of modern Science, make me appreciate, as it merits, the honour to be one of you.

In renewing my thanks, I pray you kindly accept my respectful and devoted sentiments.

(Signed) CH. FABRY.

FACULTÉ DES SCIENCES DE L'UNIVERSITÉ
DE PARIS,
SORBONNE,
December 16, 1920.

SIR,

I have the honour to acknowledge receipt of the Official Diploma of Honorary Membership of the Royal Institution, of which I was informed by a letter from Mr. Henry Young.

I shall be greatly obliged if you will kindly convey my thanks to the President, to the Board of Managers, and to the Members generally of your illustrious Society. I recognize the prize, the mark of high esteem, and the great honour which they have accorded me, and I am extremely sensible of it.

Accept, Sir, my best sentiments,

(Signed) JEAN PERRIN.

The Chairman announced that the Managers had this day awarded the Actonian Prize of One Hundred Guineas to George Ellery Hale, D.Sc. F.R.S. Hon.M.R.I., Director of the Mount Wilson Solar Observatory, for his contributions to Solar Physics.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Agricultural Research Institute, Pusa: Memoirs: Botanical Series, Vol. X. No. 6; Vol. XI. No. 1; Bacteriological Series, Vol. I. No. 9. 8vo. 1920.
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WEEKLY EVENING MEETING,

Friday, February 11, 1921.

THE HON. SIR CHARLES PARSONS, K.C.B. Sc.D. LL.D. F.R.S., Manager and Vice-President, in the Chair.

F. W. ASTON, M.A. D.Sc.,
Fellow of Trinity College, Cambridge.

Isotopes and Atomic Weights.

POSSIBLY the most important generalization in the whole history of chemistry is the Atomic Theory put forward by John Dalton in 1803, and it is a striking tribute to the shrewd intuition of that observer that of his five postulates only one seems to be in the least degree faulty, and over a century of active and unremitting investigation has been necessary to detect the flaw in that one.

The postulate in question states that "atoms of the same element are similar to one another and equal in weight." Of course, if we take this as a definition of the word "element," it becomes a truism; but, on the other hand, what Dalton meant by an element, and what we understand by the word to-day, is a substance such as hydrogen, oxygen, chlorine or lead, which has unique chemical properties and cannot be resolved into more elementary constituents by any known chemical process. For many of the well-known elements Dalton's postulate still appears to be strictly true, but for others—probably the majority—it needs some modification.

The general state of opinion at the end of the last century may be gathered from the following quotations from Sir William Ramsay's Address to the British Association at Toronto in 1897: "There have been almost innumerable attempts to reduce the differences between atomic weights to regularity by contriving some formula which will express the numbers which represent the weights with all their irregularities. Needless to say, such attempts have in no case been successful. Apparent success is always attained at the expense of accuracy, and the numbers reproduced are not those accepted as the true atomic weights. Such attempts, in my opinion, are futile. Still the human mind does not rest contented in merely chronicling such an irregularity; it strives to understand why such an irregularity should exist. . . . The idea . . . has been advanced by Prof. Schutzenberger, and later by Mr. Crookes, that what we term the

atomic weight of an element is a mean; that when we say the atomic weight of oxygen is 16 we merely state that the average atomic weight is 16; and that it is not inconceivable that a certain number of molecules have a weight somewhat higher than 32, while a certain number have a lower weight."

That such conjectures were then regarded as wildly speculative shows how strong was the faith in Dalton's postulate, which is all the more remarkable when we consider that at that time not one single direct experimental proof of it had been offered. Such proof, obviously, can only be obtained by some method which measures the masses of atoms individually, and at that time none had been developed.

The first direct evidence that the atoms of an element were at least approximately equal in mass appears to be that obtained by Sir J. J. Thomson in 1910 by his well-known method of analysis of positive rays. The fact that sharply defined parabolic streaks were obtained at all proves that the ratio of the masses of the separate particles causing them to the charges of electricity they carry is constant. The latter was known to be a definite unit, or a simple multiple of it, so that if the masses of the individual atoms varied amongst each other in an arbitrary manner an indistinct blur would result instead of a clear-cut parabola.

Before going on to consider the evidence of positive rays in greater detail it will be as well to re-state briefly the evidence upon which the theory of isotopes was founded. The first indication that it might be possible to obtain substances having identical chemical properties but different atomic weights was afforded by the brilliant researches on the radioactive elements made by Sir E. Rutherford and his colleagues. Investigations on the transformations of the different radioactive families showed that certain products, such as lead, could be formed in several ways. Each of the leads so formed was found to have chemical properties identical in every respect with those of ordinary lead, but their method of production precluded any possibility of them all having the same atomic weight. Such bodies, although having different atomic weights, must occupy the same position in the Periodic Table of the elements, and on this account have been called "isotopes" by Prof. Soddy.

Moseley's epoch-making discovery has shown us that chemical properties depend not upon atomic weight but upon something much more fundamental, namely, *atomic number*. The atomic number of an element is the number of units of positive electric charge on the nucleus of its atoms; the nuclear charge of hydrogen is 1, of helium 2, of lithium 3, and so on. We see, therefore, that isotopes are elements having the same atomic number but different atomic weights.

The theory of isotopes was triumphantly vindicated during the war by the researches of Soddy, Richards, Honigschmidt, and others, on the atomic weights of lead found in various radioactive minerals.

Quantities were obtainable which were ample for the most accurate determinations by chemical methods, and the atomic weights were found to differ from each other and from ordinary lead by quantities altogether outside possible experimental error.

Long before this convincing proof was forthcoming the theory of isotopes was discussed with the greatest interest in connection with atomic weights in general. If isotopes occurred among the heavy elements, why should they not be possible among the lighter non-radioactive ones? In which case elements with fractional atomic weights might clearly be mixtures, the constituents having atomic weights equal to whole numbers. This explanation was a very attractive one, for the curious jumble of whole numbers and fractions in the atomic weights when referred to oxygen as 16 has always been a serious stumbling-block in the way of any simple theory of atom building. The accurately determined atomic weight of chlorine 35·46 has certainly nothing to recommend it. It is reminiscent of the number of square yards in a square rod, pole or perch; but the idea of Nature, working on the same lines as the British weights and measures, is eminently unattractive.

The first support of the isotope theory among non-radioactive elements was given by the anomalous behaviour of the inactive gas neon when analyzed by Sir J. J. Thomson's method of positive rays. It is of interest to note that the announcement was made in this room by Prof. Thomson himself, and that the first sample of gas to show the effect was supplied by Prof. Sir James Dewar. This peculiarity was that whereas all elements previously examined gave single, or apparently single, parabolas, that given by neon was definitely double. The brighter curve corresponded roughly to an atomic weight of 20, the fainter companion to one of 22, the atomic weight of neon being 20·20. In consequence of reasoning adduced from the characteristics of the line 22, the discoverer was of the opinion that it could not be attributed to any compound, and that therefore it represented a hitherto unknown elementary constituent of neon. This agreed very well with the idea of isotopes which had just been promulgated, so that it was of great importance to investigate the point as fully as possible.

The first line of attack was an attempt at separation by repeated fractionation over charcoal cooled with liquid air, but even after many thousands of operations the result was entirely negative. It is some satisfaction to know that this result was inevitable, as Prof. Lindemann has recently shown, on thermodynamical grounds. Fractional diffusion through pipeclay was more effective and gave a positive result. An apparent difference of density of 0·7 per cent between the lightest and heaviest fractions was obtained after an exceedingly laborious set of operations. When the war interrupted the research, it might be said that several independent lines of reasoning pointed to the idea that neon was a mixture of isotopes,

but that none of them could be said to carry the conviction necessary in such an important development.

When the work was recommenced, attention was again turned towards positive rays, for it was clear that if an analysis could be made with such accuracy that it could be demonstrated with certainty that neither of the two atomic weights so determined agreed with

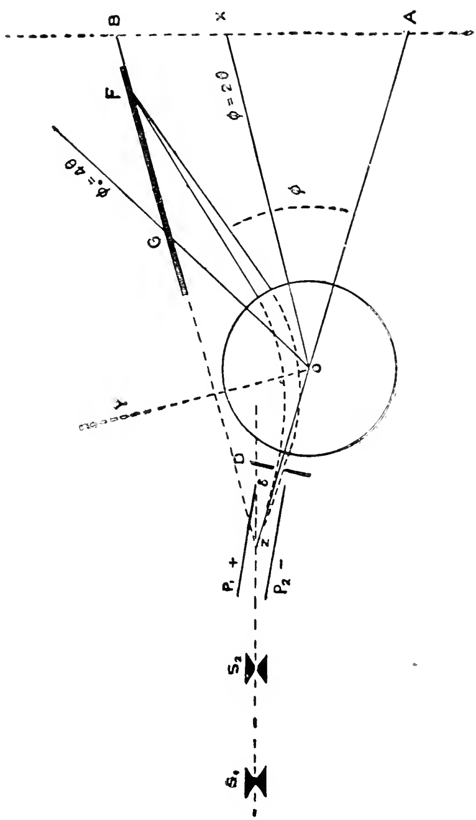

Fig. 1.

the accepted chemical figure, the matter could be regarded as settled. This could not be done with the parabolas already obtained, but the accuracy of measurement was raised to the required degree by means of the arrangement illustrated in Fig. 1. Positive rays are sorted out into a thin ribbon by means of the two parallel slits S_1 S_2, and are then spread into an electric spectrum by means of the charged plates P_1 P_2. A portion of this spectrum deflected through an

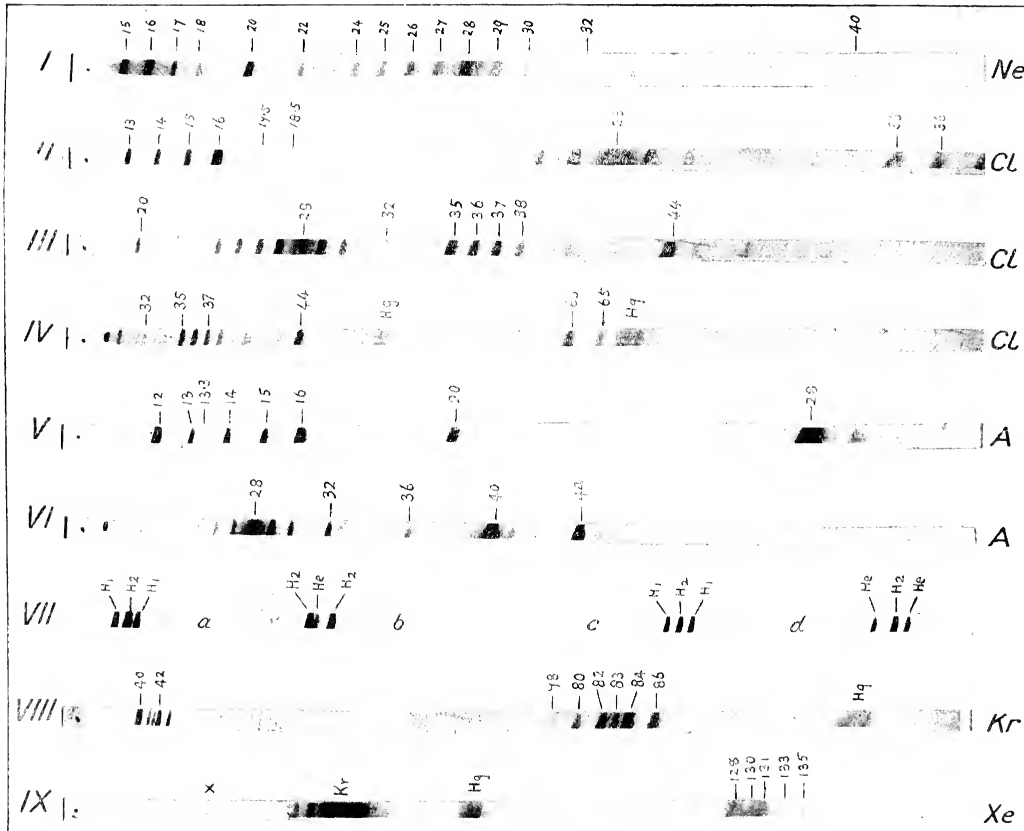

FIG. 2.

angle θ is selected by the diaphragm D and passed between the circular poles of a powerful electromagnet O, the field of which is such as to bend the rays back again through an angle ϕ more than twice as great as θ. The result of this is that rays having a constant mass (or more correctly constant m/e) will converge to a focus F, and if a photographic plate is placed at GF as indicated, a *spectrum dependent on mass alone* will be obtained. On account of its analogy to optical apparatus, the instrument has been called a positive ray spectrograph and the spectrum produced a mass-spectrum.

Fig. 2 shows a number of typical mass-spectra obtained by this means. The numbers above the lines indicate the masses they correspond to on the scale O=16. It will be noticed that the displacement to the right with increasing mass is roughly linear. The measurements of mass made are not absolute, but relative to lines which correspond to known masses. Such lines due to hydrogen, carbon, oxygen and their compounds are generally present as impurities or purposely added, for pure gases are not suitable for the smooth working of the discharge tube. The two principal groups of these reference lines are the C_1 group due to C (12), CH (13), CH_2 (14), CH_3 (15), CH_4 or O (16), and the C_2 group (24 to 30) containing the very strong line C_2H_4 or CO (28). These groups will be seen in several of the spectra reproduced, and they give, with the CO_2 line (44), a very good scale of reference.

It must be remembered that the ratio of mass to charge is the real quantity measured by the position of the lines. Many of the particles are capable of carrying more than one charge. A particle carrying two charges will appear as having half its real mass, one carrying three charges as if its mass was one-third, and so on. Lines due to these are called lines of the second and third order. Lines of high order are particularly valuable in extending our scale of reference.

When neon was introduced into the apparatus four new lines made their appearance at 10, 11, 20 and 22. The first pair are second order lines and are fainter than the other two. All four are well placed for direct comparison with the standard lines, and a series of consistent measurements showed that to within about one part in a thousand the atomic weights of the isotopes composing neon are 20 and 22 respectively. Ten per cent of the latter would bring the mean atomic weight to the accepted value of 20·20, and the relative intensity of the lines agrees well with this proportion. The isotopic constitution of neon seems therefore settled beyond all doubt.

The element chlorine was naturally the next to be analyzed, and the explanation of its fractional atomic weight was obvious from the first plate taken. Its mass spectrum is characterized by four strong first order lines at 35, 36, 37, 38, with fainter ones at 39, 40. There is no sign whatever of any line at 35·46. The simplest explanation of

the group is to suppose that the lines 35 and 37 are due to the isotopic chlorines and lines 36 and 38 to their corresponding hydrochloric acids. The elementary nature of lines 35 and 37 is also indicated by the second order lines at 17·5, 18·5, and also, when phosgene was used, by the appearance of lines at 63, 65, due to $COCl^{35}$ and $COCl^{37}$.

Quite recently it has been found possible to obtain the spectrum of negatively charged rays. These rays are formed by a normal positively charged ray picking up two electrons. On the negative spectrum of chlorine only two lines, 35 and 37, can be seen, so that the lines at 36 and 38 cannot be due to isotopes of the element. These results, taken with many others which cannot be stated here in detail, show that chlorine is a complex element, and that its principal isotopes are of atomic weight 35 and 37. There may be, in addition, a small proportion of a third of weight 39, but this is doubtful. Spectra I., II., III. and IV. show the results with chlorine taken with different magnetic field strengths.

The objection has been raised on many occasions that if chlorine consists of isotopes, how is it that its atomic weight has been determined so accurately and so consistently by different chemists? The obvious explanation of this appears to be that all the accurate determinations have been done with chlorine derived originally from the same source, the sea, which has been perfectly mixed for æons. If samples of the element are obtained from some other original source, it is quite possible that other values of atomic weight will be determined, exactly as in the case of lead.

The mass spectrum of argon shows an exceedingly bright line at 40, with second order line at 20, and third order line at 13 1/3. The last is particularly well placed between known reference lines, and its measurement showed that the triply-charged atom causing it had a mass 40 very exactly. Now the accepted atomic weight of argon is less than 40, so the presence of a lighter isotope was suggested. This was found at 36, and has now been fully substantiated; its presence to the extent of about 3 per cent is sufficient to account for the mean atomic weight obtained by density determinations.

The elements hydrogen and helium presented peculiar difficulties, as their lines were too far removed from the reference lines for direct comparison. By means of a special "bracketing" method moderately accurate values were obtained. Helium appears to be exactly 4 on the oxygen scale, but hydrogen is definitely greater than unity. The value obtained agrees very well with that already arrived at by chemical methods, namely, 1·008. At the same time, measurements of the 3 line, first observed by Sir J. J. Thomson, were made which came out at 3·024, satisfactorily proving it to be due to triatomic hydrogen.

Krypton and xenon gave surprisingly complex results, the former

consisting of six isotopes, 78, 80, 82, 84, 86. The weights of these could be determined with great accuracy by means of the excellent second and third order lines they gave. The first experiments with xenon lead to the observation of five isotopes, the provisional values of which were given as one unit too low. Owing to the kindness of Prof. Travers and Dr. Masson, I have recently been enabled to repeat the analysis with gas much richer in xenon. With this the second order lines could be observed and measured. The five principal isotopes of xenon are 129, 131, 132, 134, 136; there is apparently a faint sixth component at 128 and a doubtful seventh at 130.

Experiments with boron fluoride indicated that boron has at least two isotopes, 10 and 11, and that fluorine is a simple element of atomic weight 19.

Silicon is another unmistakably complex element having two isotopes, 28 and 29, with a possible additional one, 30.

Bromine was of great interest. As it has an atomic weight almost exactly 80, it might reasonably be expected to be simple and an isobare of one of the kryptons; actually it consists of equal parts of 79 and 81.

Sulphur, phosphorus and arsenic are all apparently simple elements. Mercury is certainly mixed, though its closer components cannot be resolved with the present apparatus. Its very characteristic groups are seen as high as the fifth order, and appear on nearly all the spectra taken. The group consists of a continuous succession of lines forming a band, 197 to 200, a strong line at 202, and a weak one at 204. Recently, at Copenhagen, Bronsted and Hevesy have succeeded in partially separating the isotopes of mercury by a fractional distillation at extremely low pressure. They give as their figures for the densities compared to normal mercury as unity—

Condensed mercury	0·999980
Residual mercury	1·000031

The error of experiment is claimed to be less than one part in a million.

Selenium, tellurium, antimony and tin have all been used in the discharge tube with no results of any value. This is unfortunate, for the atomic weight of selenium, 79·2, suggests that one of its isotopes must be an isobare of bromine or krypton; also the relation between tellurium and iodine is of great interest.

Iodine fortunately gave a very definite result. It is a simple element of atomic weight 127. This is rather surprising, for all the theoretical papers on the isotopic constitution of elements have predicted a complex iodine. Prophecy in physics becomes a difficult trade when experimental results produce these surprises, and apparently the only really reliable prediction is that there are plenty more in store for us.

The following is a list of elements and isotopes determined to date :—

TABLE OF ELEMENTS AND ISOTOPES.

Element	Atomic Number	Atomic Weight	Minimum Number of Isotopes	Masses of Isotopes in order of their intensity
H	1	1·008	1	1·008
He	2	3·99	1	4
B	5	10·90	2	11, 10
C	6	12·00	1	12
N	7	14·01	1	14
O	8	16·00	1	16
F	9	19·00	1	19
Ne	10	20·20	2	20, 22, (21)
Si	14	28·30	2	28, 29, (30)
P	15	31·04	1	31
S	16	32·06	1	32
Cl	17	35·46	2	35, 37, (39)
A	18	39·88	2	40, 36
As	33	74·96	1	75
Br	35	79·92	2	79, 81
Kr	36	82·92	6	84, 86, 82, 83, 80, 78
I	53	126·92	1	127
X	54	130·42	5, (7)	129, 132, 131, 134, 136, (128, 130 ?)
Hg	80	200·60	(6)	(197–200), 202, 204

(Numbers in brackets are provisional only.)

By far the most important result of these measurements is that, with the exception of hydrogen, the weights of the atoms of all the elements measured, and therefore almost certainly of all elements, are whole numbers to the accuracy of experiment, namely, about one part in a thousand Of course, the error expressed in fractions of a unit increases with the weight measured, but with the lighter elements the divergence from the whole-number rule is extremely small.

This enables the most sweeping simplifications to be made in our ideas of mass. The original hypothesis of Prout, put forward in 1815, that all atoms were themselves built of atoms of Protyle, a hypothetical element which he tried to identify with hydrogen, is now re-established, with the modification that the primordial atoms are of two kinds : atoms of positive and negative electricity.

Although the latter unit has long been known to us as an "Electron," its mate, which appears to be the real unit of mass, has only recently been given the name of "Proton."

The Rutherford atom, whether we take Bohr's or Langmuir's development of it, consists essentially of a positively charged central

nucleus around which are set planetary electrons at distances which are great compared with the dimensions of the nucleus itself. As has been stated, the chemical properties of an element depend solely on its atomic number, which is the charge on its nucleus expressed in terms of the unit charge e. A neutral atom of an element of atomic number N has a nucleus consisting of K + N protons and K electrons, and around this nucleus are set N electrons. The weight of an electron on the scale we are using is 0·0005, so that it may be neglected. The weight of this atom will therefore be K + N, so that if no restrictions are placed on the value of K any number of isotopes are possible.

The first restriction is that, excepting in the case of hydrogen, K can never be less than N, for the atomic weight of an element is always found to be equal to, or greater than, twice its atomic number. The upper values of K also seem to be limited, for, so far, no two isotopes of the same element have been found differing by more than 10 per cent of its mean atomic weight; the greatest numerical difference is eight units in the case of krypton. The actual occurrence of isotopes does not seem to follow any law at present obvious, though their number is probably limited by some condition of stability.

Protons and electrons may therefore be regarded as the bricks out of which atoms have been constructed. An atom of atomic weight m is turned into one of atomic weight $m+1$ by the addition of a proton plus an electron. If both enter the nucleus the new element will be an isotope of the old one, for the nuclear charge has not been altered. On the other hand, if the proton alone enters the nucleus, and the electron remains outside, an element of next higher atomic number will be formed. If both these new configurations are possible they will represent elements of the same atomic weight, but with different chemical properties. Such elements are called "isobares," and are actually known among the radioactive elements.

The case of the element hydrogen is unique, for its atom appears to consist of a single proton as nucleus with one planetary electron. It is the only atom in which the nucleus is not composed of a number of protons and electrons packed exceedingly close together. Theory indicates that when such close packing takes place the effective mass will be reduced, so that when four protons are packed together with two electrons to form the helium nucleus, they will have a weight rather less than four times that of the hydrogen nucleus, which is actually the case.

It is not to be supposed that the whole-number rule is of exact mathematical accuracy, for the unit of the oxygen scale is a "packed" proton + an electron, and its value will certainly alter slightly with the degree of packing. On this account, it is of the greatest importance to push the accuracy of methods of atomic weighing as far as possible, for variations from the whole-number rule, if they could be

determined with precision, would give us some hope of laying bare that innermost of secrets, the actual configuration of the charges in the nucleus.

The results I have described lie on the border line of physics and chemistry, and, although as a chemist I view with some dismay the possibility of eighteen different mercuric chlorides, as a physicist it is a great relief to find that Nature employs at least approximately standard bricks in her operations of element building.

[F. W. A.]

WEEKLY EVENING MEETING,

Friday, February 18, 1921.

J. H. Balfour Browne, K.C. D.L. J.P. LL.D., Manager and Vice-President, in the Chair.

Solomon J. Solomon, R.A.

Strategic Camouflage.

[Abstract.]

From so many points of view the war through which we have passed was paradoxical. We picture it as a grimy war. The noblest figure for us was a soldier in full kit covered with the sacred mud of Flanders or the Somme. But on the other side of the line the scenic artist had raised, like a canopy, acres of picturesque landscape. And although this war scenery was exposed to the daylight, and clear pictures of it were in the hands of all who were searching for the minutest indications of military activity, it remained undetected to the end. And when the end came nothing of this fairyland remained which would signify anything to anyone who had not already probed its secret. True it was hedged round by protective agencies. But, inevitable as all conversant with the technique of modern war now know it to have been, so daringly original was its conception that the efforts on its discovery (a few weeks before the most critical time in the war) to get the knowledge home to those who could use it, made no headway against the assumption that the multitude of Allied experts could not have been systematically deceived for three long years.

Everyone can take in an ordinary photograph at a glance, so it

was assumed that after an elementary lecture or two based on text-books of obscure origin, flying-men could read any war-time aerial photograph. Whereas really its accurate interpretation depends on deduction, based on a thorough knowledge of skiagraphy—the science of light and shade—such as only an experienced painter could be expected to have.

War was revolutionised by the introduction of aircraft. That had literally changed the point of view of our late enemy long before 1914.

The airship had called into being the perfected long-range camera. "The other side of the hill" had disappeared. How much the camera in favourable conditions could record had become one of the main subjects of military research.

If a few men crossed a field their movements could be traced; the work-worn earth around a camp or an aerodrome revealed its character at a glance. The shadows cast by an upright structure, nowever advertised with post-impressionist patterns, would do the same for it. Night might not last long enough to obscure the movements of an army, and if it did, and that army left the high road, daylight would show up the silver trail of the great slug and its whereabouts. If strategic intentions were in such circumstances to remain a secret there was but one solution of the problem: extensive overhead cover at selected points, on which should be reproduced the character and the chief features of the ground it concealed. And this conception was elaborated by the Germans, and reached such a pitch of artistic plausibility that no abnormality in it was detected by us. "The eye only sees what it knows."

It must not be forgotten that the Germans had prepared themselves for some years, and were responsible for most of the innovations. Our readers had had no first-hand experience. It was not till late in 1915, when we brought down a German aeroplane and developed the films found on it, that we realised the extraordinary capacity of the camera. It was after that that our scientists got to work and gave us photography as good as any the enemy had enjoyed.

But curiously enough the official text-books on which the instruction of our readers was based contained photographs far too detailed to have been prepared in war conditions with our earlier ineffective cameras.

Bernhardi wrote in his "Preparations for War," published in 1911: "We must develop the means of concealing our attacking movements"—and they did. And we have learnt that the covering art is not only the best of defensive expedients—for what is not seen is the last thing to suffer—but also the most insidious and dangerous of offensive means. It makes no noise, it gives no warning either by smell or movement—in fact, it is the only concealer of movement.

On the Western front the proportion of German losses in killed

was very little over half that of the Allies. In that safeguarding of life, art and concrete cunningly incorporated in the artistic schemes played the major part.

Years of preparation assured to the Germans the probability of fighting only in enemy country. They had no unconcealed camps. They already knew that camps of tents and huts, as we had them, could not be hidden, and the means they devised, which they call "Verkleidung von Lagern," if not discovered, would enable them to move their troops in secrecy. Those troops were billeted in the larger towns with the population, where they were immune from attacks by us. The Germans compelled the townspeople in places nearer the battle zone to remain; they knew the value of their protecting presence.

In the few incriminating photographs in my possession, strikingly enough, the covered areas were found to be located 15 kilometres (a night's march) from the larger towns and from one another, with something like Prussian precision. The troops would march from a town like Bruges by night to Aertryke, lie concealed during the daylight under an imitation aerodrome provided there, and the next night would move on to another covered camp at St. Pierre Capelle, which is 5 miles from their Nieuport front. From Courtrai, Gelewhe, another covered area is just 15 kilometres, and a few miles from the Ypres Salient. So that we may infer that this covering of extensive areas was the pivot of German strategy.

The interchange of troops would go on undetected, and concentrations be made by these means in the case of an intended offensive.

Ludendorff brought about the disastrous March surprise "by the concealment of 40 or 50 divisions in anti-aircraft shelters immediately behind his front."

When our troops followed the retreating Germans a few months later they came across this netting intact, under which Ludendorff crowded perhaps 10,000 to 12,000 men in every hundred yards square in the early morning of March 21, 1918. And these troops are described as swarming like bees out of a hive only 400 yards away. How came it about that, although by then the air authorities had been warned of the concealing methods employed by the Germans, Ludendorff's artificial landscape, dotted here and there along a 50-mile front, remained undetected? The answer is not far to seek. There were several factors; but the general lack of artistic perception was the chief, and the war by this time had become as much as anything a war of pictures.

Also what the German reviewers now admit as the "colossal extent of masking" made our authorities incredulous. Detection by the inexorable laws of light and shade went for nothing. The intelligence denied it and the difficulties involved were thought insuperable. It did not occur to them that we in peace time should

partly cover areas as large for agricultural shows which might last a week, or that the German method was more economical than our own; for the material we employ for the construction of 100 huts would, laid out in German fashion, effectively conceal 13,000 troops.

Some models were shown.

One, of an erection with sides sloping at an angle of ten degrees, which, unlike an upright structure, casts no shadow until the sun is low: the principle employed by the Germans in the lay-out of their covering material.

Another, of a cottage, was explained, known after a visit to St. Pierre Capelle, to be only 9 feet from the ground to the eaves. Its shadow was four times as wide as that of a sham pasteboard house, placed in the same parallel on top of the camouflage covering (to give it the sense of solid ground), and which, because of the narrowness of its shadow, could not have been high enough to accommodate a big dog.

Another model showed the tricks used by the camoufleur to alter the angle of shadow.

On the screen an aerial picture of real fields in harvest time was contrasted with the imitation of such fields. In the real, shorn ground appeared like velvet, and each patch of root crops or grass was clearly distinguishable, but on the hangar surfaces (which made up the picture of worked fields with sham stooks on them) there was not a sign of vegetation, and clear proof that they were composed of iron, with wide corrugations for the sake of drainage, ply board, and large areas of tarred paper. Breaks in the material were marked features in the photograph, and samples of the stuff used gathered in Flanders in 1920 were shown.

A dummy tram track on top of this roofing affected by the undulations made a serpentine line, whereas the path it imitated was shown to be quite straight. And the soil beside it is being ploughed for the first time since the German occupation in July 1921. Had the population not been deported there would have been no cessation of labour in the fields. They were returning to their ruined village in driblets, as huts were erected for them in 1921. The fixed shadows of trees painted on the road covers were two hours too early for the position of the sun, but the distorting of real shadows in their proximity gives some idea of the thoroughness of the camoufleur.

Concrete blockhouses, always the first thing put up by the Germans, were standing in 1920, where they stood in 1917, supporting the landscape roofing.

Among the pictures of the imitation landscape was a deserted Red Cross camp within five miles of Amiens. These explain the long waits between the 1918 offensives. In that inhospitable country such cover had to be devised for the men before they were brought up. Let me add that, although the military authorities refused to

believe that they had all along been misled in this matter, they now, in 1921, recognise the existence of the German method here described. And if ever unfortunately we are again engaged in a European war, the lesson to be learnt by it is that concrete, concealment, and the capacity to interpret aerial photography with accuracy, may avert such losses as were incurred in the last.

[S. J. S.]

WEEKLY EVENING MEETING,

Friday, February 25, 1921.

Colonel C. H. Grove-Hills, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

John Buchan, M.A. LL.D.

The American Civil War.

[Abstract.]

Mr. Buchan began by explaining the causes which led to the struggle, and which were deeper and more involved than the mere question of slavery. He analysed the extraordinary difficulties before Abraham Lincoln, and showed the peculiar fortitude required to make the decision for war. He then sketched the main features of the campaign. The problem of the North was first to raise armies adequate to her superior man-power, and this was eventually done by Lincoln's policy of the draft. In the second place, she had to use her navy and her economic assets to starve the enemy; and, finally, she had to find generals who could use her preponderance in numbers and material to the best purposes. Mr. Buchan showed how the policy of the "shrinking quadrilateral" ultimately won—the policy which gradually pressed in the South from four sides till Lee was driven to surrender. He concluded with character sketches of Lee and Lincoln, the latter of whom he considered the greatest man born in modern times of our common blood.

WEEKLY EVENING MEETING,

Friday, March 4, 1921.

J. H. Balfour Browne, K.C. D.L. J.P. LL.D.,
Vice-President, in the Chair.

W. A. Tait, M.Inst.C.E. M.R.I.

Severn Crossings and Tidal Power.

Several very pretty engineering problems have arisen in regard to the crossings of estuaries and rivers on the British coast.

It is interesting to note that the Thames, Forth and Tyne were in turn pioneers in less usual forms of crossing—thus :

1. The Thames was first with subaqueous tunnelling, and afterwards with a bascule bridge.

2. The Forth was first with a train ferry, and afterwards with a cantilever bridge.

3. The Tyne was first with a double-decked bridge arranged to carry both road and railway traffic.

Brunel's Thames Tunnel, between Wapping and Rotherhithe, was intended to enable road traffic to be taken directly across the Thames without going a journey of several miles up to and down from London Bridge through as busy thoroughfares as are to be found anywhere. This tunnel was driven through mud without the aid of compressed air, and was opened in 1843, after having occupied nearly eighteen years in construction, including a stoppage of about seven years owing to want of funds.

The work was carried on under the direction of the first and second of the illustrious trio of Brunels. The length of the tunnel is about a quarter of a mile, and it was completed at an overhead cost of about £1,300 per lineal yard, and was probably one of the most costly forms of construction ever carried out. The work was frequently interrupted by the Thames breaking in.

(A slide was thrown on the screen showing the measures taken to stop an inrush of water. The slide also showed men going up to the leading end of the tunnel in a boat.)

The tunnel seems only to have been completed in the first instance between the shafts on either side of the river, and to have been then used for foot passengers ; some shops were opened inside.

It was acquired in 1865 by the East London Railway Co. for

£200,000, and was brought into operation as a railway as soon as the necessary bridge approaches were completed.

(A slide was displayed showing cross-section of the two tunnels side by side. Another slide was exhibited showing the positions of other crossings of the Thames, all of a special nature, between London Bridge and Blackwall, and all of which were made after the Thames Tunnel became a railway tunnel.)

In the six miles by river below London Bridge there are now the following five crossings:—

1. The Tower Bridge, opened in 1894.
2. The Thames Tunnel, already described.
3. The Rotherhithe Tunnel, for foot passengers and vehicles, opened in 1908.
4. The Greenwich Tunnel, for foot passengers only, opened in 1902.
5. The Blackwall Tunnel, foot passengers and vehicles, opened in 1897.

No account is taken in the above list of the Tower Subway, which was opened for foot passengers in 1870, and was closed about the time that the Tower Bridge was opened, and is now used for carrying water-pipes across the Thames.

Tower Bridge.—Wolfe Barry's adoption of the bascule principle at the Tower Bridge probably carried with it the maximum improvement of facilities for crossing the river with the minimum of interruption of either river or road traffic. In the same way long and heavy approach gradients were obviated, and there was the minimum of demolition of riverside property. The bridge, which was opened in 1894, provided for great developments in cross-river traffic.

Forth Ferry.—The train ferry on the Forth was between Granton and Burntisland, a distance of 5½ miles. Being open to the North Sea it was at times exposed to very stormy seas, which even large passenger steamboats shrunk from encountering.

Prior to the construction of the train ferry it was the custom, on the arrival of the train at either end, to remove all goods from the wagons and place them in boats lying alongside the pier, and to retransfer these goods to wagons after the boats had reached the other side of the Firth. The operations involved much loss of time and great expense, and often caused breakage and damage to the goods, which was the source of frequent litigation.

One of the chief troubles to be overcome in the design of the work arose from the variation in the level of the water, there being a difference between high and low water spring tides of about 16 feet. This was overcome by the construction of a masonry slip upon which a cradle was run up or down to the required level to enable wagons to be taken on or off the lines of rails on to the steamship, which was capable of carrying about forty loaded wagons.

(A slide was exhibited showing the masonry slip and cradle.)

This ferry, which was called by Bouch, its designer, "A Floating Railway," was the forerunner of many other railway ferries in Great Britain, and in America, and in the Continent.

It was in particular the forerunner of the very useful war-time Channel ferries, i.e. between—

1. Richborough and Dunkirk.
2. Richborough and Calais.
3. Southampton and Dieppe.

The Forth train ferry continued in operation until the opening of the Forth Bridge in 1890.

THE RIVER SEVERN is crossed at Gloucester by Telford's 150-feet span-arch bridge, and is tidal to Worcester, about 27 miles higher up. The estuary below this has now only two regular means of crossing, as the ferries at Old and New Passage have been disused for many years. The two crossings are :—

1. The Severn and Wye single-line railway bridge, about three-quarters of a mile in length, which is situated about 26 miles by water below Gloucester, and was opened in 1879.

(Slides were exhibited showing several views of the Severn Railway Bridge.)

2. The Severn Tunnel, $4\frac{1}{2}$ miles in length, which is situated about 14 miles below Severn Bridge, was opened in 1886, after having occupied nearly fourteen years in construction.

Considerable attention has been directed of late to the Severn Estuary, both in connection with cross-communication and tidal power, which latter involves the construction of a barrage.

It is understood that the Great Western Railway Co. realises the inadequacy of the present crossings, and wishes to proceed at once with an additional one.

The desirability of discussing both problems together with a view to saving expense should accordingly be kept in view.

It may be recalled that the efficient co-operation of the Metropolitan Board of Works with the Metropolitan District Railway Co. resulted in the simultaneous carrying out of two very fine pieces of work between Westminister and Blackfriars Bridges, i.e. :—

1. The Thames Embankment.
2. The Metropolitan District Underground Railway.

These two works together were the means of making a great improvement at the minimum of cost to the public, and generally they present a useful example of a method to be followed when possible.

Any barrage which is made ought to be adapted to both railway and road traffic, and there should be no unnecessary ascent or descent.

It has lately been pointed out that the idea of damming the Severn is at least a century old. Telford was instructed about 1818

to report on a scheme proposed by the Gloucester and Berkeley Canal Co. There is a tradition that whilst he was engaged on this work he proposed to erect a barrage at Beachley, but no details have been left on record.

About 1843 Mr. Fuljames, an architect and county surveyor, took up the matter and prepared a water-colour drawing. He applied to the Admiralty in 1845 for permission to carry a high-level bridge across the estuary, near Beachley, and the Admiralty then appointed Mr. Walker, the well-known engineer, to report.

(Several slides were exhibited showing the extent of the proposals by Mr. Fuljames.)

A copy of Mr. Walker's Report is to be found in the Library of the Institution of Civil Engineers, and it is interesting to note what he said regarding the Old or Aust Passage so recently as 1850:—

"At and near the time of low water, the mail and passengers are landed at a small detached pier on the Aust side, and have to walk or be driven for some distance through the mud. There is a ferry steamer for crossing, but owing to the circumstances of the strong winds and the great currents, and the want of water at the landings, the passage is more frequently made in an open boat. On the whole the mode of crossing is, at the best, inconvenient and unpleasant, always the cause of considerable delay, sometimes of danger, and altogether unworthy of the almost national work which it has to perform. There is, as far as I know, no great communication in this country so bad, or further where an improvement is so much wanted; and the importance is increased by the fact of there being no bridge below Gloucester, which is 30 miles above the Old Passage, and no crossing below (except the New Passage, which is inferior to the Old) without going down the Avon from Bristol."

There is no room for doubt that the present means of cross-channel communication is very limited in extent considering the magnitude of the traffic which has to be dealt with.

The remedial methods available seem to be, in alphabetical order, as follows: (1) barrage; (2) bridge; (3) train ferry; (4) tunnel.

A properly designed barrage would—

(*a*) Enable tidal power to be developed.

(*b*) Be designed to serve both as a roadway and railway to carry all kinds of traffic across the estuary.

(*c*) Facilitate navigation and develop ports, thus fostering industries on the upper reaches.

The advantages to be claimed for a high-level bridge by itself would be, the cross-river traffic would be provided for without interference with present river traffic.

A low-level bridge with an opening span would, other things being alike, have the same advantages as a high-level bridge. It would also, however, involve only the minimum of expenditure in

the first instance, and would also afford facilities for the construction of a barrage at a later date.

A train ferry would not be a really workable proposition until after the construction of the barrage, and then it would probably not be required.

The great range of tide and the shallowness of the margins of the estuary, together with the speed of the tidal currents, are all points to be taken into account before adopting a train ferry there.

An entirely new tunnel is not a proposal to be looked upon with favour at the moment.

The gradients on the Severn Tunnel route are severe, owing to the necessity for tunnelling underneath the deepest part of the Severn, which is known as the "Shoots." The rail level of the Great Western main line is at this place about 130 feet below Ordnance datum.

The cost incurred in the construction of the Severn Tunnel and the annual cost of pumping ever since the opening in 1886, as well as the cost of working the steep gradients, have all militated against the driving of another tunnel even for railway purposes.

There is no satisfactory way of economically accommodating road, rail and pedestrian traffic all together in one tunnel, after the manner of Stephenson's Newcastle High-Level Bridge. This is a great drawback, as it is of first consequence that a new crossing should provide for all kinds of traffic.

There are also questions of ventilation to be considered in a deep subaqueous tunnel.

It may, however, be pointed out that the duplication of the up-line on the English side of the estuary—mainly in the open, though partly in tunnel—would at comparatively small cost add considerably to the traffic-carrying capacity of the tunnel.

We are, however, mainly concerned with the barrage as constructed in the first instance or at a later date.

Two considered proposals which have been put forward may now be referred to:—

1. By Messrs. Addenbrooke, Meik and Twinberrow, who favour a site near Beachley; and

2. By the Ministry of Transport, which favours a site near the Severn Tunnel at New Passage.

These proposals are to some extent on similar lines, modified by local circumstances; but before dealing with these I wish to express my thanks to Messrs. Addenbrooke, Meik and Twinberrow, and to the Advisers of the Ministry of Transport, for very kindly putting information at my disposal and generally affording me a number of facilities.

Both proposals involve:—

1. The construction of a dam across the Severn in order to utilise the valley of the river above the dam site as a reservoir.

2. The installation of low-pressure turbines, generators, etc., at the dam.

3. The filling of the reservoir by the flood tide.

4. The driving of the turbines by the issuing water during the ebb tide.

5. The construction of high-level reservoirs.

6. The equalisation of the tidal power over a fortnight by means of water pumped up to the high-level reservoirs and drawn from them at time of slack water to drive high-pressure (or secondary) turbines.

7. The construction of a lock for navigation purposes.

8. The provision of improved facilities for navigation above the barrage.

The main points of difference are in respect to the railways and roads, i.e. whether they are to be constructed—

1. At the low level with an opening span ; or

2. At the high level without an opening span.

As regards the proposed dams, it would appear that a rock foundation is available at each of the sites. It is intended that these dams should have suitably controlled openings.

The Institution of Civil Engineers is in possession of a copy of a survey made by the late Admiral Beechey, which survey has, I understand, been of considerable use to the originators of the two schemes now under discussion.

Besides the tidal water, the barrage at Beachley will have to dispose of flood water from an area of 4,500 square miles.

The barrage at the tunnel site will have to dispose of a considerably greater quantity of tidal water, together with the flood water from an additional 1,600 square miles.

The tidal current is occasionally 8 feet per second at Beachley, and practically no part of the bed of the river on the proposed line of the barrage is laid bare at low water.

The estuary at the site near the tunnel is much wider than at Beachley, and the current, in present circumstances, at high water is accordingly less. Practically the whole of the proposed barrage at the lower site can be built in the dry at low water.

The greatest depth at low water at both sites is about 60 feet, with the difference that this depth extends for a considerably greater width at the Beachley site than at the tunnel site, and with the further result that at certain states of the tide the current will be greater at the tunnel site than at the Beachley site.

The great quantity of water to be operated upon, and the limited head available, will necessitate room for many large turbines, and in consequence it has been determined that the barrage at either site should not be constructed in the shortest line across the river ; thus :

The Beachley barrage is proposed to be 9,800 feet in length, instead of about 5,700 feet.

The barrage near the tunnel is proposed to be 25,000 feet in length, instead of about 11,500 feet.

The additional length will add considerably to the cost. On the other hand, the long dam, with proper sluices and weirs, will facilitate the upward passage of tidal water and the downward passage of tidal and flood waters, subject always to the eddies that may be formed where the currents have to change their direction.

The configuration of the land adjoining the estuary will necessitate adequate provision for disposing of the surplus water when the River Severn, or the Rivers Severn and Wye, as the case may be, happens to be in flood at or near the time of high water.

One special reason for keeping this point in view is that the proposed method of working will, in point of fact, prolong the time of high water very considerably more than at present, and so increase the tendency to flooding.

(A slide was exhibited showing the proposed method of working.)

There are records of very large floods having taken place during the last 150 years, but in one of the most recent, i.e. during the construction of the Severn Tunnel, the actual height of the tide was found to be 10 feet above the calculated height for that night.

While all proper care must be taken at either site to prevent flooding of low-lying land, it is particulaly necessary to guard against any flooding of the Severn Tunnel for two reasons :—

1. The Severn Tunnel is much the more important of the present railway crossings of the estuary, and must not be interrupted.

2. From 12 to 30 million gallons of water have to be pumped from the tunnel daily according to the weather conditions.

This water, though fairly hard, was described by Prof. Percy Frankland as of an extraordinarily high degree of bacteriological purity and of great organic purity. It may become very valuable when there is plenty of power available in the neighbourhood.

With a comparatively small and varying head, only slow speeds will be attained by the turbines, and this will involve considerable expenditure upon machinery.

Both schemes depend to a great extent on the satisfactory working of large turbines with comparatively low falls. It appears, therefore, to be of first consequence that such turbines should be installed and tried under as nearly as possible similar conditions.

There appear to be several suitable places in Scotland where such turbines could be effectively tested before the erection of the Severn Barrage has been decided upon. One likely place is Loch Seaforth, which is an arm of the sea near Stornoway, and which has an inner loch running nearly at right angles to the outer loch, with a rather rapid race near the point of junction.

The construction of a comparatively short barrage near this junction would facilitate the use of the storage in the inner loch, which has a surface area of nearly one square mile. The rise at

spring tides is about 13 feet. If the scheme is a success, so much the better for Stornoway and district, where developments are in hand.

If, however, the scheme should for any reason fail, then the small expenditure upon it would prevent the expenditure of a very large sum upon the Severn works.

The proposed test at Loch Seaforth would after all only be a test with clean water. Some experiments should also be made with Bristol Channel water before any decision is come to.

As regards the filling of the reservoir above the Severn Barrage on the flood tide, it can hardly be anticipated that the spring and neap tides will in future rise to quite the same height as formerly, on account of the obstruction to be interposed.

This observation applies to both sites. It should also be pointed out that the lines suggested for the lengthening of the dams may cause eddies in the neighbourhood at or about the time of half tide, when, however, comparatively few, if any, vessels will be passing through the barrage.

As regards the driving of the turbines by the issuing water during the ebb tide, it has still to be ascertained what wearing effect the silt in suspension will have upon the moving parts of the turbines. This also refers to both schemes.

The originators of both schemes have come to the conclusion that it will be more profitable to work the turbines on the ebb tide only.

One of the technical papers, in reviewing the scheme of the Ministry of Transport, points out that "the portions of the dam coming on either side of the 'Shoots' would be used for the installation of turbines and electric generators, the 'Shoots' channel itself forming the tail race." The paper then goes on to say that, "As vessels desiring to proceed into or away from the shipping basin, or up or down the river, would have to pass through the 'Shoots' in order to reach the system of locks which it is proposed to construct at the far end, they would have to run the gauntlet of turbine discharges on both quarters. . . . The effect of such discharges in a channel only a little wider than the Thames at Westminster on vessels, which would of course have to be navigated at slow speed, might well be considered."

The paper apparently overlooks the fact that the turbines will not be at work at or near the time of slack water, which vessels will in all probability use for passing through the barrage.

(Slides were exhibited to show the following :—(1) Bird's-eye view ; (2) the River Wye, looking towards the Severn ; (3) island near Beachley, looking towards England ; (4) Upper Wye.)

As regards the construction of high-level reservoirs, great care must of course be taken in proving the foundations, having regard to the trouble encountered during the construction of other reservoirs in the neighbourhood. The effect of accumulated Severn Estuary water on the surrounding country will also have to be considered.

Care would of course have to be taken in the arrangements of reservoir works to make certain that the inlet and outlet of the high-level reservoirs were kept far apart in order to facilitate the settlement of silt before the water was drawn off to the secondary turbines.

Time alone will show whether the moving parts of the secondary turbines will wear quickly with Severn Estuary water. It would, however, be of advantage that pumping experiments should be made for a year or two with a view to ascertaining whether there would be a sensible diminution of the available storage caused by the deposit of silt.

It has been suggested that the secondary or high-pressure turbines should be driven by water free from silt, and such a provision, if it could be effected economically, would no doubt be found of advantage for prolonging the life of the centrifugal pumps also.

It should be kept in view that the originators of both schemes propose that the water level above the barrage shall never in future be drawn down so low as at present. This should improve the quality of the water to be lifted by the centrifugal pumps at the expense possibly of certain other interests.

(A slide was exhibited to show the equalisation of the tidal power over a fortnight.)

The holding up of the water level above the barrage will be an important feature in facilitating the use of the estuary above the barrage for navigation purposes, and great developments on either side of the estuary, above the barrage, may be looked for.

Below the barrage there are enormously valuable dock interests on both sides of the channel, and it is only reasonable that the probable effect of the construction of the proposed barrage should be most carefully considered, and, if necessary, adequate measures adopted so that these interests may not be prejudicially affected.

Formerly it was the custom to deride tidal power mainly on account of its several inconveniences :—

1. The amount of power available varied from day to day.
2. The maximum power was not available at the same time every day.

Sir George Darwin was somewhat outspoken on the subject in his classic work on "The Tides."

"It has been supposed by many that when the coal supply of the world has been exhausted, we shall fall back on the tides to do our work, but a little consideration will show that, although this source of energy is boundless, there are other far more accessible funds on which to draw.

"I saw some years ago a suggestion that the rise and fall of old hulks on the tide would afford serviceable power. If we picture to ourselves the immense weight of a large ship, we may be deluded for a moment into agreement with this project, but numerical calculation soon shows its futility. The tide takes about six hours to rise from

low water to high water, and the same period to fall again. Let us suppose that the water rises ten feet, and that a hulk of 10,000 tons displacement is floating on it, then it is easy to show that only twenty horse-power will be developed by its rise and fall. We should then require ten such hulks to develop as much work as would be given by a steam-engine of very moderate size, and the expense of the installation would be far better bestowed on water-wheels in rivers, or on windmills. I am glad to say that the projector of this scheme gave it up when its relative insignificance was pointed out to him. It is the only instance of which I ever heard where an inventor was deterred by the impracticability of his plan.

"We may then fairly conclude that with existing mechanical appliances, the attempt to utilise the tide on an open coast is futile; but where a large area of tidal water can be easily trapped at high water, its fall may be made to work mill-wheels or turbines with advantage. The expense of building long jetties to catch the water is prohibitive, and therefore tide mills are only practicable where there exists an easily adaptable configuration of shoals in an estuary. There are no doubt many such mills in the world, but the only one which I happen to have seen is at Bembridge, in the Isle of Wight. At this place embankments formed in the natural shoals are furnished with lock-gates, and enclose many acres of tidal water. The gates open automatically with the rising tide, and the incipient outward current, at the turn of the tide, closes the gates again so that the water is trapped. The water then works a mill-wheel of moderate size. When we reflect on the intermittence of work from low water to high water, and the great inequality of work with springs and neaps, it may be doubted whether this mill is worth the expense of retaining the embankments and lock gates.

"We see then that, notwithstanding the boundless energy of the tide, rivers and wind and fuel are likely for all time to be incomparably more important for the use of mankind."

It is suggested that the Severn is better adapted to the production of power than any other known British river on account of :—

1. The high rise of tide.

2. The considerable body of water above the site of the proposed dam.

One objection to the proposed high-level bridge at the Beachley site is that it would be necessary to carry the rails over the Rivers Wye and Severn at an elevation of about 140 feet above Ordnance datum. This would entail a fairly long gradient on the English side leading up to the bridge, and a still longer gradient on the Welsh side.

Objection is taken to the suggested provision of an opening bridge on the low-level line at the tunnel site. This, however, is partly answered by the fact that until there is a barrage, shipping generally, but particularly small craft, can only make very limited

use of the upper estuary on account of the great tidal range and the currents there.

Further, the locking-basin in the barrage is proposed to be of considerable capacity for ships, and in any case a loop line of railway, with another opening bridge, will be provided on the upstream side of this basin, and so prevent interruption of cross-river traffic.

These points are to be kept in view for the protection of the river and dock authorities and shipowners, who, however, should not be allowed to override the reasonable demands of the public and the railway companies and road authorities for proper and convenient river facilities.

As between the railway and road authorities on the one hand, and the dock and river authorities on the other, I am of opinion there will be little difficulty in obtaining Parliamentary sanction for a bridge at either site in the first instance.

It should be first determined, however, whether the Beachley site or the Severn Tunnel site is the better one for the complete scheme, taking all the really important circumstances into account.

The next point to be considered is whether the bridge is to be at the low level, with an opening span, or whether it is to be a high-level bridge.

It should be pointed out, in the first instance, that the Beachley site is not very suitable for a low-level bridge, unless at least a second opening span is made to permit the passage of river traffic on the Wye.

On the other hand, the site near the Severn Tunnel is not suitable for a high-level bridge on account of the expense that would be incurred in making approaches thereto. No matter which site is chosen, the bridge ought to be designed for both railway and road traffic in the first instance, with the intention of afterwards being used to facilitate the erection of a barrage after the necessary Parliamentary authority has been obtained.

Operations can be started in connection with the bridge as soon as the site has been definitely chosen and the design approved. This would enable additional cross-channel traffic to be handled at the earliest date, while affording facilities for the construction of the barrage when authorised.

The French Government have a scheme for the utilisation of 750,000 horse-power on the Rhine, between Basle and Strasbourg. Britain need not shrink from a proposal for 500,000 horse power installation for a ten-hour working day on the Severn.

Notwithstanding the careful consideration which has already been given by Messrs. Addenbrooke, Meik and Twinberrow; Mr. Boving and Mr. Britton: Mr. Dana, Sir George Darwin and Mr. Davey; Sir Alexander Gibb and Messrs. Ferguson, Menzies and Maunsell, of the Ministry of Transport; Prof. Gibson, Mr. Liversedge, Captain Taylor and Prof. Unwin—I am strongly of opinion that, having regard to the enormous importance as well as to the novelty of the

proposal, a great deal of investigation must still be performed before the country would be justified in proceeding to carry out a scheme for the utilisation of the Severn tidal power.

In respect, however, that the navigation of the Upper Severn might be greatly developed, and an abundant supply of cheap power made available at a number of industrial centres, or at similar centres to be created on the margin of the Severn Estuary, it would appear that the possible result is sufficiently promising to justify the Government spending a considerable sum of money for the purpose of investigation and experiments.

It is hoped that during the time these experiments are proceeding, the home manufacturers will devote themselves to the preliminary design and preparations for the construction of the required turbines, etc.

(Several additional slides were displayed, showing the "bore" on the Severn and various waterways leading therefrom.)

[W. A. T.]

GENERAL MONTHLY MEETING,

Monday, March 7, 1921.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Sir Frank Benson, LL.D.
Isaac William Bullen,
Mrs. R. C. Bussell,
Miss E. Caird,
Charles Harvey Combe,
Hayward Radcliffe Darlington, J.P. M.A.
Paul Faraday,
George J. Goldie, L.D.S.
Major Gordon Home,
Walter Gibb Klein,
Alfred Goodman Levy, M.D.
Miss Julia Lindley,
Evan MacRury, M.A.
Mrs. Alexander Ractivand,
Sir Robert Robertson, K.B.E. F.R.S.
Mark R. Trower,
Miss Lucy Western,

were elected Members.

The Secretary reported the decease on February 17 of Professor William Odling, and on February 21 of Professor L. C. Miall, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to place on record their sense of the great loss the Institution, and the Science of Chemistry, have suffered by the death of William Odling, Bachelor of Medicine, Waynflete Professor of Chemistry, Oxford, 1872–1912, Fellow of the Royal Society, Fellow of the Royal College of Physicians, Past President of the Chemical Society and of the Institute of Chemistry.

Professor Odling succeeded Faraday as Fullerian Professor of Chemistry of the Royal Institution in 1868, during Tyndall's occupancy of the Chair of Natural Philosophy, and was a Member for nearly half a century.

His valuable contributions to Chemistry are embodied in numerous communications to the Memoirs of Scientific Societies, and his published works include "Manual of Chemistry," "The Chemical Changes of Carbon," and "Practical Chemistry." He delivered eleven courses of Day Lectures, including three of the Christmas Courses of Juvenile Lectures originally initiated by Faraday, and the following Friday Evening Discourses :—

Graham's Recent Discoveries on the Diffusion of Gases ... 1867
The Absorption of Gases by Metals 1867
The Decomposition of Water and the Oxy-Hydrogen Flame 1868
The Simplest Organic Compounds 1869
The Ammonia Compounds of Platinum 1870
The Production of Chlorine 1871
The Revived Theory of Phlogiston 1871
The New Metal Indium 1872
The History of Ozone 1872
Evaporation and Diffusion 1873
The Paraffins and their Alcohols 1876
The New Metal Gallium 1878
Sir B. C. Brodie's Researches on Chemical Allotrophy 1882
The Dissolved Oxygen of Water 1884

The present Fullerian Professor, in the year 1898, summarized the scientific position of Odling in the following words:—

"The work of Odling has been an essential factor in the development of modern Chemistry. It is characterised by precise and clear ideas, and an almost forensic ability for putting things in a straight, concise and unembarrassing manner. His early labours in advancing the development of the newer Chemistry deserve our warm gratitude, and his many published works and addresses on Organic and Inorganic Chemistry, together with his translation of the work of Laurent, have all been of material service in diffusing a knowledge of our science. The papers he has contributed on Chemical Notation and on the question of types all display a marvellous precision as well as elegance of thought. Everyone must admit the debt of gratitude we owe him for his iconoclastic labours in clearing out old and vague notions, and for the courageous manner in which he supported the newer ideas of his time."

On behalf of the Members the Managers desire to express their deepest sympathy with the Odling family in their bereavement.

Resolved, That the Managers desire to place on record their sense of the loss which the Royal Institution, and the Sciences of Biology and Natural History, have sustained by the death of Louis Compton Miall, Doctor of Science, Fellow of the Royal Society, Emeritus Professor of Biology, University of Leeds.

Dr. Miall was an original general investigator in Palæontology, and his studies on comparative anatomy, especially those on the skull of the Crocodile and on the Indian Elephant, are classical examples of his work.

He held the office of Professor of Biology in the Yorkshire College of Science in 1876, and to his loyal and devoted services its eventual establishment as the University of Leeds is largely due. His sound judgment and business aptitude were of inestimable value during the early years of its existence, and its success to-day has been determined to a great extent by his influence and personality.

Dr. Miall was elected in 1904 Fullerian Professor of Physiology in the Royal Institution, and delivered several courses of Day Lectures on his Biological Researches, and two Friday Evening Discourses on:—

The Surface Film of Water and the Life of Plants and Animals (1892).

A Yorkshire Moor (1898).

On behalf of the Members the Managers desire to express their deepest sympathy with the Miall family in their bereavement.

The following Lecture Arrangements After Easter 1921 were announced:—

Professor R. A. Sampson, F.R.S., Astronomer Royal for Scotland. Two Lectures on 1. Present Position of the Nebular Hypothesis; 2. The Measurement of Starlight. On *Tuesdays*, April 5, 12.

Professor Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S. M.R.I., Fullerian Professor of Physiology, Royal Institution. Four Lectures on Darwin's Theory of Man's Origin (In the Light of Present Day Evidence). On *Tuesdays*, April 19, 26, May 3, 10.

Edward Clodd, J.P., Author of "Magic in Names," etc. Two Lectures on Occultism: Its Origin and Development. On *Tuesdays*, May 17, 24.

Sir James Frazer, D.C.L. LL.D. F.B.A. F.R.S. M.R.I. Two Lectures on 1. Roman Life (Time of Pliny the Younger); 2. London Life (Time of Addison). On *Tuesdays*, May 31, June 7.

C. T. R. Wilson, D.Sc. F.R.S., Observer in Meteorological Physics, Solar Physics Observatory. Two Lectures on Thunderstorms. (The Tyndall Lectures.) On *Thursdays*, April 7, 14.

H. S. Foxwell, F.B.A., Fellow and Director of Economic Studies, St. John's College, Cambridge. Two Lectures on Nationalisation and Bureaucracy. On *Thursday*, April 21, and *Wednesday*, April 27.

Charles S. Myers, M.D. Sc.D. F.R.S. M.R.I., Director, Psychological Laboratory, Cambridge. Two Lectures on Psychological Studies: 1. The Localisation of Sound; 2. The Appreciation of Music. On *Thursdays*, May 5, 12.

D. S. MacColl, LL.D., Keeper of the Wallace Collection. Two Lectures on War Graves and Monuments. On *Thursdays*, May 19, 26.

Sir Alexander C. Mackenzie, Mus.Doc. D.C.L. LL.D. M.R.I., Principal, Royal Academy of Music. Two Lectures on Beethoven (with Musical Illustrations). On *Thursdays*, June 2, 9.

H. H. Dale, C.B.E. M.D. F.R.S., Head of Department of Biochemistry and Pharmacology under Medical Research Council. Two Lectures on Poisons and Antidotes. On *Saturdays*, April 9, 16.

Henry Yule Oldham, M A. F.R.G.S., University Lecturer in Geography, Cambridge University. Two Lectures on The Great Epoch of Exploration: 1. Portugal; 2. Spain. On *Saturdays*, April 23, 30.

E. C. C. Baly, C.B.E. F.R.S., Professor of Inorganic Chemistry, University of Liverpool. Two Lectures on Chemical Reaction. On *Saturdays*, May 7, 14.

Francis Legge, F.S.A. M.R.I. Two Lectures on Gnosticism and the Science of Religions. On *Saturdays*, May 21, 28.

Robert S. Rait, C.B.E. LL.D., Historiographer Royal for Scotland. Two Lectures on 1. Scotland and France; 2. Scott and Shakespeare. On *Saturdays*, June 4, 11.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Geological Survey, Records, Vol. LI. Part 2. 8vo. 1920.
Scientific Reports of the Agricultural Research Institute, Pusa, 1919–20. 8vo.
Agricultural Journal, Vol. XV. Part 6. 8vo. 1920.
Memoirs: Department of Agriculture, Bacteriological Series, Vol. I. No. 3. 8vo. 1920.
Archæological Survey: New Imperial Series, Vol. XXIX. fol. 1920.

Aeronautical Society, Royal—Journal, Feb. 1921. 8vo.

American Geographical Society—Geographical Review, Jan. 1921. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXXI. Nos. 2–3. 8vo. 1920–1.

Australia, Commonwealth, Institute of Science and Industry—Science and Industry, Vol. II. No. 11. 8vo. 1920.

Bankers, Institute of—Journal, Vol. XLII. Part 3. 8vo. 1921.
Batavia, Royal Observatory—Verhandelungen, No. 6. 8vo. 1920.
Belgium, Royal Academy of Sciences—Bulletin, 1920, Nos. 9–11. 8vo.
Mémoires, in 8vo Second Series, Tome V. Fasc. 6–8. 1920.
Birmingham and Midland Institute—Report for 1920. 8vo. 1921.
The Unguarded Boundary. By H.E. The Hon. J. W. Davis. 8vo. 1920.
Boston Public Library—Bulletin 4 Ser. Vol. II. No. 4. 8vo. 1920.
Botanic Society, Royal—Quarterly Summary, Jan. 1921. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXVIII. Nos. 8 9. 4to. 1921.
British Astronomical Association—Journal, Vol. XXXI. No. 4. 8vo. 1921.
British Dental Association—Journal, Vol. XLII. No. 4. 8vo. 1921.
Canada, Department of Mines—Bulletin, No. 32. 8vo. 1920.
Phosphate in Canada. 8vo. 1920.
Chemical Industry, Society of—Journal, Feb. 1921. 8vo.
Chemical Society—Journal and Proceedings, Feb. 1921. 8vo.
Chemistry, Institute of—Journal and Proceedings, Feb. 1921. 8vo.
Scientific Aspects of Tanning. By J. T. Wood. 8vo. 1920.
Civil Engineers, Institution of—Proceedings, Vol. CCVII. 8vo. 1920.
Clarke, Messrs. J. W. (The Publishers)—Hampton's Scholastic Directory, 1920–21. 8vo. 1921.
Colonial Institute, Royal—United Empire, Vol. XII. No. 3. 8vo. 1921.
Devonshire Association—Report and Transactions, Vol. LII. 8vo. 1920.
Editors—Animals' Defender, March 1921. 8vo.
British Engineers' Journal, Feb. 1921. 4to.
Chemist and Druggist, Feb. 1921. 8vo.
Church Gazette, Feb. 1921. 8vo.
Dyer and Calico Printer, Feb. 1921. 4to.
Engineer, Feb. 1921. fol.
Engineering, Feb. 1921. fol.
Engineering Production, Feb. 1921. 4to.
Junior Mechanics, Feb. 1921. 8vo.
Law Journal, Feb. 1921. 8vo.
London University Gazette, Feb. 1921. 4to.
Model Engineer, Feb. 1921. 8vo.
Musical Times, Feb. 1921. 8vo.
Nation and Athenæum, Feb. 1921. 4to.
Nature, Feb. 1921. 4to.
Nuovo Cimento, Jan. 1921. 8vo.
Physical Review, Feb. 1921. 8vo.
Science Abstracts, Jan. 1921. 8vo.
Terrestrial Magnetism, Vol. XXV. No. 4. 8vo. 1920.
Wireless World, Feb. 1921. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIX. No. 296. 1921. 8vo.
Franklin Institute—Journal, Vol. CXCI. No. 2. 8vo. 1921.
Geographical Society, Royal—Journal, Vol. LVII. No. 3. 8vo. 1921.
Geological Society of London—Abstracts of Proceedings, Nos. 1065–67. 8vo. 1921.
Horological Institute—Horological Journal, Feb. 1921. 8vo.
Hutchinson, Rev. H. N. (The Author)—Extinct Monsters and Creatures of Other Days. Revised edition. 8vo. 1910.
Illuminating Engineering Society—Illuminating Engineer, Oct.–Dec. 1920. 8vo.
Indian Association for the Cultivation of Science—Proceedings, Vol. V. Part 2. 8vo. 1920.
Report for 1918. 8vo. 1920.
Leeds Chamber of Commerce—Year Book, 1920. 8vo.
Legge, F., Esq., M.R.I. (The Translator) — Hippolytus Philosophumena. 2 vols. 8vo. 1921.
Linnean Society—Proceedings: 132nd Session. 8vo. 1920.
List of Fellows, 1920–21. 8vo. 1920.

London County Council—Gazette, Feb. 1921. 4to.
London Society—Journal, Feb. 1921. 8vo.
Meteorological Office—Daily Readings, Dec. 1920. 4to.
Meteorological Society, Royal—Quarterly Journal, Vol. XLVII. No. 197. 8vo. 1921.
National Physical Laboratory—Collected Researches, Vol. XV. 4to. 1920.
New York, Society for Experimental Biology—Proceedings, Vol. XVIII. Nos. 2-4. 8vo. 1920-1.
Numismatic Society, Royal—Numismatic Chronicle, 1920, Parts 3-4. 8vo.
Onnes, Dr. H. Kamerlingh—Communications from the Physical Laboratory of the University of Leiden, No. 152. 8vo. 1921.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, Jan. 1921. 8vo.
Parker, W. R., Esq., M.D. M.A. M.R.I.—Hampstead Heath: By Members of the Hampstead Scientific Society. 8vo. 1913.
Peru, Cuerpo de Ingenieros de Minas—Bolletin, Nos. 96-97, 99. 8vo. 1919-20.
Pharmaceutical Society of Great Britain—Journal, Feb. 1921. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLV. No. 3. 8vo. 1921.
Physicians, Royal College of—List of Fellows, etc., 1921. 8vo.
Radcliffe Library, Oxford—Catalogue of Books, 1920. 8vo. 1921.
Rio de Janeiro, Observatorio Nacionale—Annuario, 1921. 8vo.
Rome, Ministry of Public Works—Giornale del Genio Civile, Jan. 1921. 8vo.
Röntgen Society—Journal, Vol. XVI. No. 66, Jan. 1921. 8vo.
Royal Engineers' Institute—Journal, Vol. XXXIII. No. 3. 8vo. 1921.
Royal Society of Arts—Journal, Feb. 1921. 8vo.
Royal Society of London—Philosophical Transactions, A, Vol. CCXXI. No. 592; B, Vol. CCX. No. 381. 4to. 1921.
Proceedings: A, Vol. XCVIII. No. 694. 8vo. 1921.
Royal Society of New South Wales—Journal and Proceedings, Vol. LIII. 8vo. 1919.
Sanitary Institute, Royal—Journal, Vol. XLI. No. 4. 8vo. 1921.
South Africa, Union of—Journal of Agriculture, 1921, Nos. 1-2. 8vo.
Statistical Society, Royal—Journal, Vol. LXXXIV. Part 1. 8vo. 1921.
United States Department of Agriculture—Experiment Station Record, Vol. XLIII. No. 9. 8vo. 1921.
Journal of Agricultural Research, Vol. XX. Nos. 4-6. 8vo. 1920.
United States Naval Observatory—Annual Report, 1920. 8vo.
United States Patent Office—Official Gazette, Vol. CCLXXXII. No. 3; Vol. CCLXXXIII. No. 2. 8vo. 1921.
Washington, National Academy of Sciences—Proceedings, Vol. V. No. 11. 8vo. 1920.
Western Australia, Agent-General—Quarterly Statistical Abstract, Sept. 1920. 8vo.

WEEKLY EVENING MEETING,

Friday, March 11, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

JOHN FREEMAN, M.D.

Medical Idiosyncracies.

[NO ABSTRACT.]

WEEKLY EVENING MEETING,

Friday, March 18, 1921.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Manager and Vice-President, in the Chair.

Sir Frederick Bridge, C.V.O. M.A. Mus.Doc.,
King Edward Professor of Music, University of London.

Researches of a Musical Antiquarian.

(With Musical Illustrations.)

[Abstract.]

Dr. Wheatley, in an article on London in the time of Shakespeare, says, "The Cries in the streets were much the same as those recorded in the 15th Century." (From "London Luckpenny.") He goes on to mention particularly the Cry of "What d'ye lacke?" and Rock Sampire, Old Boots, Buy a Mat, Small Coal, Green Brooms. Also he mentions the Cries outside the prison doors for the "poor women in the dark dungeons," and "Bread and meat for the tender mercy of God to the poor prisoners of the Marshalsea." He would have been much interested in the complete collection of these old Cries, which are preserved to us in the works of three great musicians who were contemporaries of Shakespeare.

So far as I can learn these three compositions have not hitherto been printed. They are preserved in manuscript in the British Museum, and my attention was called to them some time ago, when that great and useful antiquary, Dr. Southgate, was helping me with regard to a work by Dering called "Country Cries." Dr. Southgate was actually working at Weelkes' composition when death overtook him, to the great loss, not only of myself, but of this and all kindred societies.

I am anxious you should hear all the music I have to put before you, and therefore must cut my remarks as short as possible.

We get very little information of these Cries from the musical historians. Burney tells us nothing. Hawkins gives us a few interesting particulars, though he is not very correct in certain matters. He says, "It was formerly a practice with the musicians to set the Cries of London to music, retaining the very musical

notes of them." He goes on to say, "Orlando Gibbons set music in four parts to the Cries in his time." This sentence needs explanation. When Hawkins states that the composers set the Cries to music, one naturally concludes that they took the words of the old Cries and set them to music. But he continues, "retaining the very musical notes of them." Now this is exactly what is so interesting. The examples which I hope to put before you will show that the composers did not set the Cries, but incorporated the old Cries prevalent in London in Shakespeare's time, words and music, into a new form of composition for voices and instruments. This new form is very striking since it is an enlargement of the scope of the Fancy.

This was the great instrumental form of the time, composed as a rule for strings, and consisting, for the most part, of elaborate work of a contrapuntal nature, full of points of imitation. Nearly all the composers of the period of which I am speaking wrote Fancies. Even up to the days of Purcell they were in vogue, Purcell himself writing some remarkable ones, before he ventured on the new form of Sonatas. But these Fancies had no vocal parts, and what I am now concerned with is a delightful development of the Fancy—not only containing the usual parts for strings, but adding vocal parts, these vocal parts consisting exclusively of the Old London Cries, with their words and the original music; not new settings by the composer of the Fancy. That these old Cries are given to the original music is proved by the fact that the three composers whose Fancies I have been able to get together, Weelkes, Gibbons and Dering, used the same words and music for the various Cries.

The first specimen I give to-day is the least elaborate and shortest. The Cries in Weelkes are allotted to one part only, the Cantus or Melody. But for variety's sake and to save the singers fatigue, I have allotted the various Cries to various singers and different voices. I have, however, made no change in the music. One point I ought to explain. In the middle of the movement the Fancy makes a pause—the time changes—and a charming little dance tune to some odd words—"Twinkledowne Tavye" is introduced. This is exactly what we find in the Ballets, and is the only approach to the Madrigal style in the composition, with, perhaps, the exception of the conclusion with its Alleluia, which sounds rather like an Anthem! The seller of brooms has a charming song—not a Cry, but a song—such as the itinerant vendors of ink, blacking, garlick, etc., sang, and which are found in the more elaborate things by Gibbons and Dering. In an old play, "A right excellent and famous Comedy," entitled "Three Ladies of London," printed in 1584, one of the characters called "Conscience" enters with brooms and sings this very song:—

"Have you any old boots or any old shoes,
Pouch rings or buskings—will ye buy any brooms."

This was no doubt sung to this fine old tune.

In the case of the dance and the song I have arranged the words for five voices to the music written for the viols. But of course the Melody only may be sung—the other parts being in the accompaniment which is arranged from the score for viols.

In the three Fancies which I have scored there are nearly 150 different Cries, and itinerant vendors' songs. All three composers have used some of the Cries, and two out of the three a great number. It is extraordinary what a collection it makes. There are thirteen different kinds of fish, but the Cries are very similar in some cases.

18 Different kinds of Fruit.
6 Kinds of Liquors and Herbs.
11 Vegetables.
14 Kinds of Food.
14 Kinds of Household Stuff.
13 Articles of Clothing.
9 Tradesmen's Cries.
19 Tradesmen's Songs.
4 Begging Songs for Prisoners and Bedlam.
1 Town Crier. (Used by all three composers.)
5 Watchman's Songs.

(Weelkes illustration was given here.)

The work of Gibbons is longer and more elaborate than Weelkes', and it is particularly interesting to find it in the form of an In Nomine! Perhaps I need not explain to this audience what an In Nomine is. But I might just say it is a form of Fancy which contained in one part a Plain-song Melody. It is wonderful how these old composers worked their florid parts round the steady-going part of monotonous Plain-song. I have had several performed at my Gresham Lectures, but they have all been for instruments only. Now here is one in which the inner viol part plays the Plain-song (a bit of ecclesiastical music), while the vocal parts are exclusively composed of the old Cries. Perhaps I had better quote old Roger North, who seems, I think, to give a more lucid account of the In Nomine than other writers. He says, "Before the introduction of Fancies whole Consorts for instruments of 4, 5 and 6 parts were solemnly composed, and with wonderful art and variation, while one of the parts (commonly in the middle) bore only the Plain-song throughout, and I guess that in some time little of other Consort music was coveted or in use." But that which was styled "In Nomine" was yet more remarkable, for it was only descanting upon seven notes with which the syllables "In Nomine Domini" agreed. And of this kind I have seen whole volumes of many parts.

This, then, is the form of Gibbons. The opening of Gibbons is very picturesque. We feel we are in the quiet street in the very

early morning. The viols have a solemn little passage of Imitation and the monotone of the Watchman is heard, going his round.

"God give you good morrow, my masters, past three o'clock and a faire morning." Then suddenly, with a change in the accompaniment from minor to major, the Cries of the vendors of fish begin. Curiously enough, Gibbons begins with mussels and not oysters—oysters come later. I will not comment on the Cries. You will hear them. Only I want to call your attention to one or two points.

There is a delightful little song—not a cry—for the seller of ink, then a very comical Town Crier, who cries a mare lost "on the 30th day of February." Then a very interesting Begging Song for the inmates of Bedlam. "Poor, naked Bedlam Tom's a cold. A small cut of thy bacon, or piece of thy sow's side, good Besse. God Almighty bless thy witts!"

Shakespeare has used many of these very words in "King Lear." "Bless thy poor wits—Tom's a cold"—(Edgar disguised as a madman in "King Lear," Sc. IV. Act 3). This Fancy is divided into two parts. Part one finishes with a delightful bit of Harmony. The most striking song in part two is for the Chimney Sweep, and this song occurs note for note and word for word in Dering's work, showing it to be a really popular tune. The work concludes with the Watchman's warning, "Twelve o'clock, look well to your locks, your fire and your light, and so good night." This beautiful specimen, and Dering's, are in a manuscript, copied in 1616, by Thos. Myrrell, so that we may be sure they were composed in Shakespeare's lifetime. The translation of the title-page is as follows: "A remedy against sadness—select songs of various authors, and on various subjects, set down by the labours and hand of Thomas Myrrell, A.D. 1616."

(Gibbons illustration.)

The Dering I present is called "What d'ye lacke?" after the opening, which is clearly intended to represent the 'Prentices at the shop doors advertising their wares. But the great charm of Dering's work is the number of real songs, i.e. more than Cries, which he preserves for us.

We have here the sellers of Ink, Old Doublets (the same as Gibbons, but here in a different time), Rosemary and Bays, Chimney Sweep (the same as Gibbons), Blacking, a Cooper (very interesting, as the tune is the first part of "Heartease," alluded to by Shakespeare), Rats and Mice, Garlick; and we also find the Begging song "Bread and Meat for the Prisoners of the Marshalsea," and other similar petitions. The whole work is an advance on Gibbons and Weelkes, and shows Dering to be a really admirable composer. I must confess I feel a great satisfaction in unearthing this specimen, because I have already, as some of you know, restored to life and use some of this composer's splendid Motets. It is to me astounding

how this great man should have been so long neglected by musicians and by historians.

His Motets are really almost as advanced as much of Purcell's music. But I have not time now to dwell upon that. I am sure you will be glad to hear this Fancy. Like the others it begins with a short contrapuntal symphony, and is scored for five viols and four voices.

There are a few very humorous Cries in this Fancy—the Corn Cutter, and the Dentist, who calls himself by the encouraging name of "Kindheart, the tooth-drawer."

[J. F. B.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1921.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Vice-President, in the Chair.

Samuel D. Bles,
Stenton Thomas Covington,
Mrs. Lancelot W. Dent,
Brian G. Donne,
Charles Godfrey, M.A. M.V.O.
Commander A. C. Goolden,
Arthur Mallalieu,
Mrs. William Marshall,
Edmund W. T. L. Brewer Williams, J.P. D.L.
Sir John Wormald, K.B.E.

were elected Members.

The Secretary reported the decease of the Rt. Hon. Lord Moulton on March 9, 1921, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to place on record their sense of the great loss the Institution and the community have sustained by the death of the Rt. Hon. Lord Moulton of Bank, Lord of Appeal, K.C.B. G.B.E. LL.D. F.R.S., Commandeur de la Legion d'Honneur, and other Foreign Orders.

Lord Moulton had a distinguished University career, graduating at St. John's, Cambridge, in 1868, with the two highest honours in the Mathematical and Physical Departments, namely, Senior Wrangler and First Smith's Prizeman. He was elected a Fellow and Lecturer of Christ's College, Cambridge. Entering the Middle Temple in 1868, he was called to the Bar in 1874, where he quickly achieved success.

A Member of the Royal Institution for forty-four years, he occupied during this period the Offices of Vice-President, Manager and Visitor. Two Friday Evening Discourses were delivered by him in the years 1876–7, the first on "Verification of Modern Scientific Methods," and the second, "Matter and Ether."

In association with William Spottiswoode, M.A. LL.D. F.R.S., who had been for many years Treasurer of the Royal Institution, and who was subsequently promoted to the Presidency of the Royal Society, he conducted a series of experimental enquiries on Electric Discharges in Rarefied Gases,

describing the sensitive conditions of Stratification, and the Gas movements under such conditions, the results of which are embodied in the following Papers published by the Royal Society:—

Sensitive State of Electrical Discharges through Rarefied Gases, 1879-1880 (Phil. Trans., Vol. 170).

Sensitive State of Vacuum Discharges, 1880–81 (Phil. Trans., Vol. 171).

Stratified Discharges, 1881 (Roy. Soc. Proc., Vol. 32).

Movement of Gas in "Vacuum Discharges," 1882 (Roy. Soc. Proc., Vol. 33).

This entrancing subject of enquiry was about this time attracting the attention of other distinguished scientific Members of the Royal Institution, notably Gassiott, de la Rue and Crookes, and their investigations may be said to lead up to the epoch-marking experiments of Sir J. J. Thomson.

After the year 1882, Lord Moulton abandoned original experimental enquiry, and devoted his exceptional talents to the solution of forensic problems. Identifying himself with the Chancery Department connected with Patent Litigation, his wide scientific knowledge soon became an endowment of utility in his position at the Bar; he rose rapidly, taking Silk in 1885. Returned for Parliament in 1885 and in subsequent elections, he was for ten years a Member of the House of Commons. In 1906 he was appointed a Judge of the Court of Appeal, and six years later was created a Lord of Appeal in Ordinary.

Lord Moulton accepted the post of Director-General of Explosive Supplies in the Ministry of Munitions when the war broke out, and in this office his powers of organisation and knowledge of chemical and physical and engineering problems were of the utmost value to the nation. The production of synthetic colouring matters derived from Coal Tar products were essentially related to the work of this department, and in this direction Lord Moulton used his best efforts to establish a British Dyestuff Industry on a sound and secure basis.

On behalf of the Members the Managers desire to express to the Moulton family their deepest sympathy with them in their bereavement.

Sir J. J. Thomson having resigned the Chair of Physics, the Managers unanimously resolved to recommend to the Members that Sir J. J. Thomson be nominated as Honorary Professor of Natural Philosophy, for election at the next General Meeting on May 9.

The Managers unanimously resolved to recommend to the Members that Sir Ernest Rutherford, D.Sc. LL.D. F.R.S., be nominated as Professor of Natural Philosophy, for election at the General Meeting on May 9, at the same salary as his predecessors.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Agricultural Journal, Vol. XVI. Part 1. 8vo. 1921.

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Index of Language Names. By G. A. Grierson. 4to. 1920.

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Aeronautical Society, Royal—Journal, March 1921. 8vo.
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Australia, Commonwealth of—Science and Industry, Dec. 1920. 8vo.
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Chemical Industry, Society of—Journal, March 1921. 8vo.
Chemical Society—Journal and Proceedings, March 1921. 8vo.
Deberain, H.—Bibliographie Scientifique Française, 1920, Fasc. 1-6 (Sect. I.-II.). 8vo. 1920.
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Chemical News, March 1921. 8vo.
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Engineering, March 1921. fol.
Junior Mechanics, March 1921. 8vo.
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Musical Times, March 1921. 8vo.
Nation and Athenæum, March 1921. 4to.
Nature, March 1921. 8vo.
New Church Magazine, March-April 1921. 8vo.
Nuovo Cimento, Feb.-March 1921. 8vo.
Physical Review, March 1921. 8vo.
Science Abstracts, Feb. 1921. 8vo.
Wireless World, March 1921. 8vo.
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Imperial Institute—Bulletin, Vol. XVIII. No. 3. 8vo. 1920.
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London University—Gazette, March 1921. 4to.
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Photographic Society, Royal—Journal, N.S., Vol. XLV. No. 4. 8vo. 1921.
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Swiss Chemical Society—Helvetica Chimica Acta, Vol. IV. Fasc. 2. 8vo. 1921.
Tôhoku Imperial University, Japan—Science Reports, Vol. IX. No. 6. 8vo. 1920.
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Experiment Station Record, Vol. XLIV. Nos. 1–2. 8vo. 1821.
United States Patent Office—Official Gazette, Vol. CCLXXXIII. No. 3–Vol. CCLXXXIV. No. 1. 8vo. 1921.
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WEEKLY EVENING MEETING,

Friday, April 8, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

R. H. A. PLIMMER, D.Sc.,
Biochemist, Rowett Research Institute of Animal Nutrition,
University of Aberdeen,
and North of Scotland College of Agriculture.

Quality of Protein in Nutrition.

[ABSTRACT.]

OF the three main groups of compounds composing our normal diet, protein is the only one which shows any marked variation. The carbohydrates and fats are converted during digestion into a few simple compounds, such as grape sugar, glycerin and fatty acids. Protein is also converted during digestion into simple compounds, but their number and variety is large, and different amounts of them arise from the different proteins. We can thus speak of quality of protein, but not of quality of carbohydrate and fat.

Our usual classification of proteins already indicates their differences, but the variety is really far greater. We need only refer to their chemical analysis. Fischer and Kossel and their pupils have shown that proteins on hydrolysis break down into some eighteen or twenty amino acids. These numerous products can be arranged for convenience into eight groups :—

I.—*Simple Mono-amino Acids.*

Glycine $CH_2(NH_2).COOH$

Alanine $CH_3.CH(NH_2).COOH$

Valine $(CH_3)_2 : CH.CH(NH_2).COOH$

Leucine $(CH_3)_2 : CH.CH_2.CH(NH_2).COOH$

Isoleucine $(CH_3)(C_2H_5) : CH.CH(NH_2).COOH$

II.—*Mono-amino Dibasic Acids.*

Aspartic Acid $HOOC.CH_2.CH(NH_2).COOH$

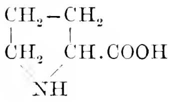
Glutamic Acid $HOOC.CH_2.CH_2.CH(NH_2).COOH$

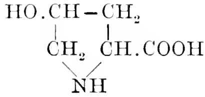
III.—*Hydroxy-amino Acids.*

Serine $CH_2OH.CH(NH_2).COOH$

Hydroxyglutamic Acid $HOOC.CH_2.CHOH.CH(NH_2).COOH$

IV.—*Heterocyclic Acids.*

Proline 

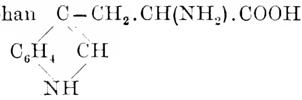
Hydroxyproline 

V.—*Mono-amino Acids with Aromatic Nucleus.*

Phenylalanine $C_6H_5.CH_2.CH(NH_2).COOH$

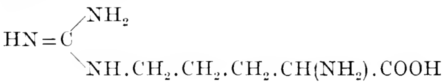
Tyrosine $HO.C_6H_4.CH_2.CH(NH_2).COOH$

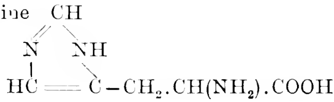
VI.—*Mono-amino Acid with Indole Nucleus.*

Tryptophan 

VII.—*Hexone Bases, or Di-amino Acids.*

Lysine $H_2N.CH_2.CH_2.CH_2.CH_2.CH(NH_2).COOH$

Arginine 

Histidine 

VIII.—*Thio-amino Acid.*

Cystine $HOOC.CH(NH_2).CH_2S-SCH_2.CH(NH_2).COOH$

The chemical analysis of the proteins shows that the various proteins yield different amounts of the amino acids. Some of the data are shown in Table I. The chief peculiarity is pointed out by enclosing the figure in a square. In general, the albumin group of proteins contains all the amino acids, except glycine, in various proportions. The globulin group is similar, but contains glycine, and has in addition a higher amount of glutamic acid, especially those globulins of vegetable origin. The phosphoproteins resemble the albumins with no striking preponderance of any single amino acid. The gliadin group of cereal proteins is peculiar in its high content of glutamic acid and proline. The members of the sclero-protein group (horn, hair, gelatin) are heterogeneous; and here we may note that silk fibroin is composed mainly of three mono-amino acids, and is the very antithesis of sturin (the protein of fish sperm), which is made up of the three hexone bases with no or very little

mono-amino acids. Gelatin lacks cystine, tyrosine, and tryptophan. Hair is richest in cystine. These are simply some of the most obvious differences.

Our analytical data are far from complete; in no case do the totals of the amino acids add up to 100. The incompleteness is chiefly due to the great difficulty of separating and estimating the

Table I.

	Ox Muscle Protein	Casein	Lact-albumin	Gelatin	Wheat Gliadin	Wheat Glutenin	Maize Zein	Maize Glutenin	Edestin	Sturin
Glycine	2·1	0	0	$\boxed{19\cdot3}$	0	0·9	0	0·3	3·8	
Alanine	3·7	1·5	2·5	3·0	2·0	4·7	9·8		3·6	
Valine	0·8	7·2	0·9		3·4	0·2	1·9		+	
Leucine	11·7	9·4	$\boxed{19\cdot4}$	6·8	6·6	6·0	$\boxed{19\cdot6}$	6·2	$\boxed{20\cdot9}$	
Phenylalanine	3·2	3·2	2·4	1·0	2·4	2·0	6·6		3·1	
Tryosine	2·2	4·5	0·9	$\boxed{0}$	1·2	4·3	3·6	3·8	2·1	
Serine		0·5		0·4	0·2	0·7	1·0		0·3	
Cystine		?		$\boxed{0}$	0·5	0·02			0·3	
Proline	5·8	6·7	4·0	10·4	$\boxed{13\cdot2}$	4·2	9·0	5·9	4·1	
Hydroxyproline		0·3		6·4					2·0	
Aspartic Acid	4·5	1·4	1·0	1·2	0·6	0·9	1·7	0·7	4·5	
Glutamic Acid	15·5	15·6	10·1	1·8	$\boxed{43\cdot7}$	$\boxed{23\cdot4}$	$\boxed{26\cdot2}$	12·7	18·7	
Tryptophan	+	1·5		$\boxed{0}$	1·0	+	$\boxed{0}$	+	+	
Arginine	7·5	3·8	3·2	9·3	3·2	4·7	1·6	7·1	$\boxed{14\cdot4}$	58·2
Lysine	7·6	6·0	$\boxed{9\cdot2}$	5·0	0·2	1·9	$\boxed{0}$	3·0	1·7	12·0
Histidine	1·8	2·5	2·1	0·4	0·6	1·8	0·8	3·0	2·4	12·9
Ammonia	1·1	1·6	1·3	0·4	5·2	4·0	3·6	2·1		
Total	67·5	66·5	57·0	65·4	83·0	59·72	85·4	45·7	81·9	83·1

individual amino acids. There may be still some unknown amino acids in small quantities: hydroxyglutamic acid has been discovered recently by Dakin by a new extraction method. This method may yet lead to new results; once again it has proved that every new process in connection with the chemistry of the proteins has given a valuable result.

Rather too great stress has been laid upon the analytical figures.

The methods hardly give exactness as far as the decimal figure, and it would have been better if the data had been returned to the nearest whole number. Many workers still give their data to two places of decimals, so that an entirely wrong impression is given of the accuracy of the method. Fischer pointed out that his method was not quantitative, but others have neglected this important statement.

The figures for the hexone bases are more accurate; it is still not sufficient to express results to two decimal places. Kossel considers that the hexone bases form a special nucleus on account of their presence in all proteins. We might value a protein by its content of hexone bases, but it is not sufficient, because their total only tells us about a third or less of the whole molecule.

Tryptophan, discovered by Hopkins and Cole, is perhaps the most important unit in the protein molecule. It is not estimated except by direct isolation, a method which is laborious and requires considerable skill. Its amount is not known except in casein and a few other proteins. By its distinctive colour reaction with glyoxylic and sulphuric acids it can readily be proved to be a constituent of most proteins.

The amount of cystine in proteins is only known in a few cases, but its amount can be gauged by the sulphur content of the protein. It is the one unit known which contains sulphur, but there are indications that there is another sulphur-containing unit.

The differences in proteins are not confined to such quantitative data; they are still more involved. Fischer's synthetical work with the amino acids has proved that the amino acids are combined together in a polypeptide form, i.e. the amino group of one amino acid is combined with the carboxyl group of another, the amino group of this acid being united with the carboxyl group of still another. We therefore consider a protein molecule to be a chain of amino acids, thus—

$$HN.CH_2.CO-NH.CH(CH_3).CO-HN.CH(C_4H_7).CO-HN.CH(C_3H_5).CO-$$

This method of combination allows theoretically of endless variation. If we take three amino acids, we can arrange them in six different ways—

glycyl-alanyl-tyrosine	alanyl-tyrosyl-glycine
glycyl-tyrosyl-alanine	tyrosyl-glycyl-alanine
alanyl-glycyl-tyrosine	tyrosyl-alanyl-glycine

With 18 or 20 amino acids the number of arrangements is almost infinite.

Differences in arrangement may be the cause of differences in proteins. Two proteins may perhaps have exactly similar amounts of amino acids and yet be different. The interchange of one amino

acid would express a difference. We may imagine the proteins of the blood or milk of different species to differ in the arrangements of the units. The one may have the arrangement such as a–b–c–d–e–f–, the other d–a–b–f–e–c–.

Another important difference may be in the so-called tautomerism of the amino acids and polypeptides. With the same arrangement of the amino acids we may have several formulæ representing the polypeptide structure :—

$$\mathrm{H_2N.CH_2.COOH}$$
$$\mathrm{H_2N.CH_2.CO-HN.CH_2.CO-HN.CH_2.COOH}$$
$$\mathrm{H_2N.CH_2.CO-HN.CH_2.C(OH){:}N.CH_2.COOH}$$
$$\mathrm{H_2N.CH_2.C(OH){:}N.CH_2.CO-HN.CH_2.COOH}$$
$$\mathrm{H_2N.CH_2.C(OH){:}N.CH_2.C(OH){:}N.CH_2.COOH}$$

$$\mathrm{H_3N.CH_2.COO}$$
$$\mathrm{H_3N.CH_2.CO-HN.CH_2.CO-HN.CH_2.COO}$$
$$\mathrm{H_3N.CH_2.CO-HN.CH_2.C(OH){:}N.CH_2.COO}$$
$$\mathrm{H_3N.CH_2.C(OH){:}N.CH_2.CO-HN.CH_2.COO}$$
$$\mathrm{H_3N.CH_2.C(OH){:}N.CH_2.C(OH){:}N.CH_2.COO}$$

Certain of the properties of the polypeptides can be explained on this basis.

Fischer's and Kossel's work has revolutionized our conception of protein nutrition. We no longer think, like Liebig and later investigators, that the protein of the food becomes directly the protein of the body, for it has been demonstrated by the physiologists that the protein of the food undergoes hydrolysis during digestion to amino acids, that the amino acids circulate in the blood, and that the tissues receive amino acids from which they build up their protein. Proteins must be regarded as a mixture of amino acids.

We can look upon a protein as we look upon the contents of a box of assorted biscuits, arranged in rows and in layers of various kinds. Each biscuit should be connected to its neighbour so that we have a continuous chain. The general appearance of the contents of two boxes is different; in one case we may find sugary biscuits on the top, in another case plain ones.

In the process of digestion the protein is acted upon by acid in the stomach with the formation of metaprotein. No great chemical change occurs, but we can imagine that the change consists in a tautomeric rearrangement in preparation for the action of the pepsin. Pepsin hydrolyses the protein at certain junctions, forming proteoses and peptones. Their formation can be compared to the separation of the layers of the biscuits. Pancreatic and the further digestion which follow in the intestine separate the individual amino acids or

biscuits entirely. The separate parts circulate to the tissues; the tissues select the ones they require and form another arrangement of the units, or simply replace those which have been used in their metabolism. Digestion and metabolism are a sort of re-shuffling of the units. In the absence of any particular unit the tissue can no longer rebuild its substance and consequently suffers. The old example of the inadequacy of gelatin is now explained. The tissues require tryptophan, tyrosine and cystine; gelatin cannot provide them.

The protein molecule can be represented better by a series of coloured blocks—the body colour representing the particular group to which the unit belongs, and the ends being white and black to represent the amino or carboxyl groupings. White is always joined to black, and a very long chain can be imagined. This can be made into a compact model by supposing that there is a tautomeric grouping at certain points; it would cause a folding back of the chain at these points or a connection to another row of units. The separation of blocks would be like the formation of proteoses and peptones; the separation of the individual pieces would be like complete hydrolysis.

In nutrition there are essentially two problems to study—the formation of new tissues as in the growth of young animals, and the maintenance of tissue, which undergoes so-called wear and tear, in adult animals. In the latter case we have ultimately to ascertain if every unit of the molecule breaks down, or certain selected units only. If these are in the middle of a chain, it would follow that the whole molecule would undergo metabolism and not units at the ends alone. The problem resolves itself into ascertaining the function of each amino acid.

Since the practical difficulties of feeding animals with a mixture of pure amino acids are far too great, advantage may be taken of feeding incomplete proteins and adding to them the missing unit or units.

Wilcock and Hopkins made the first experiment of this kind in 1906. They selected zein as protein and fed it to mice, in one set alone, in another set with the addition of 2 per cent of its amount of tryptophan. Young mice on zein alone immediately began to lose weight and generally died in 16 days; decline in weight also occurred in the other set, but with the added tryptophan death did not occur till the 30th day. Adult mice lived 27 days without tryptophan, 49 days with tryptophan. Tryptophan had thus an appreciable effect on the survival period of the animals. Zein is incomplete in respect of other units, and death was probably on this account. The experiment was repeated in 1916 by Ackroyd and Hopkins under different but better conditions. The animals were first given a mixture of amino acids from casein (i.e. without tryptophan, which is destroyed in hydrolysis by acid) to which

tryptophan was added; on the 13th day the tryptophan was omitted and included once more on the 35th day. There was growth during the first period, decline in weight during the second period followed by growth on inclusion once more of the tryptophan.

Similar experiments have been made by Osborne and Mendel in America. They used gliadin of wheat as protein. This protein is a complete one, but it contains very little of certain amino acids, especially lysine. Adult rats were maintained for quite long periods —as long as 500 days—but young rats capable of growth, though maintained for long periods, failed to grow.

We may here notice that though the growth of the animal may be suppressed and it reaches maturity in age, the capacity to grow is not lost. Osborne and Mendel illustrated this by a photograph of a rat which had failed to grow for 273 days, but resumed growth on being given a suitable diet. The small amount of lysine in gliadin led the authors to regard this unit as essential for growth. In a later experiment they added lysine at intervals; growth occurred with the lysine, but not without it.

The effect of lysine on growth was again demonstrated by Buckner, Nollau and Kastle in the case of chickens living under the natural conditions of a poultry farm. The birds were fed upon grain mixtures of high and low lysine content. As their photographs showed, more rapid growth took place on the mixture of high lysine content.

The element sulphur is present in proteins in the form of cystine, though it is possible another sulphur-containing unit is present. Little or no cystine in a protein has also an effect upon the growth of rats. This has been most clearly demonstrated in the case of the protein, phaseolin, of the navy bean. There was slow growth with this protein alone, but normal growth if the protein were supplemented with 2 per cent of its amount of cystine. Casein is deficient in cystine. Less casein is required in a diet for producing normal growth, if extra cystine be included. Fifteen per cent casein was required by itself, but only 9 per cent if cystine were added.

The amino acids containing aromatic nuclei are probably essential units of the protein, but it is difficult to carry out a decisive experiment, since all proteins contain phenylalanine, though they may lack tyrosine. There is plenty of evidence that phenylalanine can be transformed in the body by oxidation; both tyrosine and phenylalanine yield homogentisic acid in cases of alkaptonuria. Totani has shown that the almost complete removal of tyrosine from the mixture of units yielded by casein made no difference to the growth of rats. There was evidently enough phenylalanine for all purposes.

The two hexone bases—arginine and histidine—as shown by Ackroyd and Hopkins, are inter-related in nutrition. Absence of both causes loss of weight; absence of either alone lessens the rate

of growth. These two workers further showed that these amino acids are connected with the production of the purine ring in the animal body, i.e. with the production of uric acid.

The function of the whole group of mono-amino acids has yet to be determined. Are they all necessary? For glycine we can say that it is not essential, as it is the only amino acid which the animal can synthesise.

These results remind us of the well-known experiments on the need by plants of all the inorganic elements. Sir Daniel Hall, in his book on "Fertilisers and Manures," gave a striking picture of barley grains grown on a full food and foods lacking one constituent.

We may thus correlate the amino acid content of proteins for growth of animals with the set of inorganic elements needed for the growth of plants.

The relative value of various proteins in nutrition has been studied by Osborne and Mendel. In their experience lactalbumin is superior to casein, and casein to edestin. They found that 50 per cent more casein and 90 per cent more edestin were required to produce the same gain in weight; in other terms, a food containing 9 per cent lactalbumin was equal to one with 12 per cent casein and 15 per cent edestin.

Suitable mixtures of proteins have also been tested, and attempts are being made to find out the most suitable addenda for making the proteins of cereals more adequate for the growth of animals, that is, adding what we may call "good" protein to "bad" protein to make the latter efficient as food. Leaf and seed proteins are good as a mixture.

Economically it may be better to use an expensive protein as food, and produce rapid growth, than to feed for longer periods on poor proteins and get slower growth.

A simple calculation brings out the problem to be solved. We may wish to build up the casein of milk with 16 per cent of glutamic acid, and we are provided with wheat gliadin with over 40 per cent of this unit. There is waste of glutamic acid. Gliadin further contains 0·2 per cent of lysine, whilst casein contains 6 per cent. To produce this amount we require thirty times as much gliadin, and consequently the waste of glutamic is further increased.

Cannibalism is the most economical method of protein nutrition, as the amino acids of the food are in the exact proportion required by the tissues. The nearest parallel to this is the nursing of the young animal by its mother: the child actually gets the proteins of the mammary glands.

Recent work shows that quality of protein is most probably the primary cause of the disease Pellagra, although there are some indications that general insufficiency of protein, together with improper salt supply, are contributory factors.

Pellagra is a peculiar disease characterised by severe disturbance

of the whole digestive tract, by skin lesion, usually bilaterally symmetrical, and often mistaken at first for sunburn or chapping of the hands, face and neck, and other exposed areas. The nervous system is also affected.

There is no definite record of pellagra in Europe before maize was introduced into Spain by Columbus. From Spain the disease spread to France, Lombardy, and eastwards, wherever maize was extensively used for food in the poorer agricultural districts. The relation of maize to the disease puzzled the medical profession for nearly 200 years, as the disease occurred where maize was not used, and in some districts maize was used, but there was no pellagra. Roussel, in 1866, showed that it could be cured by good food, and Lorentz (1914) and Willets (1915) successfully treated advanced cases with a generous diet. Goldberger also cured and prevented the seasonal appearance of pellagra in lunatic asylums and orphanages by increasing the quantity of meat and milk; previously the diet had been deficient in this respect.

Goldberger, by the offer of a free pardon from the Governor of Mississippi, was enabled to obtain eleven convicts as volunteers for a feeding experiment to determine if pellagra could be produced by an unbalanced diet in healthy white men. The "pellagra squad," as they were called, were fed on white wheat flour, various maize preparations, polished rice, sugar, sweet potatoes, pork fat, cabbage and turnip tops. The food had an energy value of 2950 calories, and was amply sufficient in this respect. After the second month on this diet the men complained of weakness, headache, abdominal pain and other minor discomforts. After five months six developed a rash, which was pronounced by experts to be identical with that seen in pellagra. During the last four weeks all the prisoners had shown marked loss of weight, and were much out of health. Pellagra would probably have developed in the remainder, but the experiment had to be abandoned owing to the refusal of the men to continue. A control was carried out at the same time; their diet contained some meat, eggs and butter-milk; there was not a single case of pellagra, and no progressive loss of body weight.

These and other facts clearly point to the diet as the controlling factor in the cause and prevention of the disease. The determining factor seems to be the quality of the proteins. Good evidence on this point has been furnished by Wilson, of Cairo. In 1916 pellagra broke out in a camp for Armenian refugees at Port Said. Wilson showed that the diet at first supplied was inadequate both in energy supply (2200 calories) and in protein supply; 92 per cent of the protein was of vegetable origin, three quarters from wheat, and one quarter from maize.

By determining the nitrogen balance in man on various proteins, Thomas has demonstrated that proteins have very different biological values. He assigned a set of comparative values according to the

quantity required to maintain a man without loss of nitrogen and body weight. They were :—

Ox meat	104
Cow's milk	100
Fish	95
Casein	70
Rice	88
Potato	79
Peas	56
Wheat flour	40
Maize meal	30

The biological value of meat is therefore three times that of maize.

Wilson calculated that the diet as given to the refugees was equal to 22 gm. of casein. On improvement to a casein equivalent of 41 gm. no more cases of pellagra occurred.

Chick and Hume (1920) succeeded in producing in three monkeys symptoms very like those of human pellagra. The diet was very carefully selected, and was deficient only in respect that it contained no animal protein. One monkey refused the food after a short time; he lost weight, and showed signs of incipient pellagra. The second monkey also lost weight, but the loss was lessened by adding tryptophan ; but other amino acids lacking in maize had no appreciable effect. This monkey had signs of pellagra, and was cured by giving a normal diet. The third monkey had its loss of weight arrested by including tryptophan and hexone bases. This monkey showed some of the characteristic symptoms of pellagra, such as the symmetrical bilateral rash.

It appears thus that pellagra is caused by a continuous shortage in the supply of certain amino acids in the food. A diet containing animal protein in small quantities will supply the needful amino acids; a large supply of vegetable protein may not contain the needful amino acids.

[R. H. A. P.]

WEEKLY EVENING MEETING,

Friday, April 15, 1921.

Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

Ernest Law, C.B.

Wolsey as War Minister.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, April 22, 1921.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Manager and Vice-President, in the Chair.

SIR JAMES WALKER, D.Sc. LL.D. F.R.S., Professor of Chemistry, University of Edinburgh.

Electrosynthesis in Organic Chemistry.

[ABSTRACT.]

THE decomposition of water into oxygen and hydrogen by means of the electric current was effected by Nicholson and Carlisle in 1800, and affords the first example of electrolysis. Davy applied the electrolytic method to the decomposition of many compounds, and finally succeeded in isolating the alkali metals, potassium and sodium, by the electrolysis of fused potash and soda.

Faraday, his successor in the chair of the Royal Institution, laid the foundations of our theoretical knowledge of the subject. He studied in detail the nature and proportions of the products obtained by electrolysis, reduced the manifold experimental data to a simple system, and invented the nomenclature employed at the present day. The process of electrolysis in aqueous solution is conceived by him as follows: When two conducting plates connected with the opposite poles of a battery are immersed in a conducting solution, negative electricity travels towards the positive plate (*positive electrode, anode*), and positive electricity travels towards the negative plate (*negative electrode, cathode*). The electricity does not travel alone, but in association with a material carrier or *ion*, the *anion* or negatively charged ion moving towards the anode, whilst the *cation* or positively charged ion moves towards the cathode. Chemically equivalent quantities of the ions bear equal charges of electricity. When the ions reach the electrodes they are there discharged, and may then act (1) upon each other, (2) upon the water in which they are dissolved, or (3) upon the material of the electrodes. Thus, if we electrolyse sulphuric acid H_2SO_4, which yields the ions $2H^+$ and SO_4^{--}, we have at the negative electrode an action which may be represented by the equation

$$2\,H = H_2 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

two discharged hydrogen ions combining with each other to form a molecule of hydrogen gas. If the positive electrode is of copper we have the reaction

$$SO_4 + Cu = CuSO_4 \quad . \quad . \quad . \quad (2)$$

the discharged iron acting on the material of the electrode and forming copper sulphate. If, on the other hand, the positive electrode consists of the resistant metal platinum, the discharged ion acts on the solvent water according to the equation

$$2\,SO_4 + 2\,H_2O = 2\,H_2SO_4 + O_2 \quad . \quad (3)$$

sulphuric acid being regenerated and oxygen gas evolved.

Equation (1) expresses a kind of synthesis, two atoms originally separate uniting to form a single molecule. In organic chemistry synthesis in the strict sense is held to mean the union of carbon atoms originally belonging to separate molecules. The first electro-synthesis was effected in 1849 by Kolbe, who, to take a simple example, electrolysed potassium acetate solution with platinum electrodes, and at the anode obtained the hydrocarbon ethane. The negative ion of the acetates is represented by the formula $CH_3 \cdot COO^-$, and under appropriate conditions two of these when discharged at the anode interact in accordance with the equation

$$2\,CH_3 \cdot COO = CH_3 \cdot CH_3 + 2\,CO_2 \quad . \quad (4)$$

Here an organic synthesis in the strict sense has been effected, two CH_3 groups originally contained in different molecules being now joined together by their carbon atoms to form a molecule of the hydrocarbon ethane $CH_3 \cdot CH_3$. This method of synthesis was for long neglected, but attention was again drawn to it in 1890 by a suggestion of Professor Crum Brown, which was worked out in detail in conjunction with the lecturer. If the sodium ethyl salt of a dibasic acid, e.g. malonic acid $COOH \cdot CH_2 \cdot COOH$, is electrolysed, the anion $COOEt \cdot CH_2 \cdot COO^-$ reacts according to equation (4) as follows:—

$$2\,COOEt \cdot CH_2 \cdot COO = COOEt \cdot CH_2 \cdot CH_2 \cdot COOEt + 2\,CO_2.$$

The product, diethyl succinate, contains two CH_2 groups instead of the single CH_2 group present in malonic acid, and may readily be converted into sodium ethyl succinate, which yields the anion $COOEt \cdot CH_2 \cdot CH_2 \cdot COO^-$. This on electrolysis reacts as before, thus:—

$$2\,COOEt \cdot CH_2 \cdot CH_2 \cdot COO =$$
$$COOEt \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot CH_2 \cdot COOEt + 2\,CO_2$$

giving diethyl adigate, which now contains four CH_2 groups. This may be again converted into a sodium ethyl salt, and the process continued.

In this way it was possible to build up, starting from malonic acid, acids containing chains of 2, 4, 8 and 16 CH_2 groups. Starting from acids containing 3, 5 and 7 CH_2 groups, acids were prepared containing 6, 10 and 14 CH_2 groups, and with 6 CH_2 groups the acid with 12 CH_2 groups was prepared. Many acids with branched hydrocarbon chains have also been obtained by the method. In its simple form the electrosynthesis necessarily yields hydrocarbon chains with an even number of carbon atoms, a process of doubling taking place at each stage. However, acids containing an odd number of carbon atoms can be produced by the electrolysis of mixtures. Thus the acid containing 7 CH_2 groups has been prepared by the electrolysis of a mixture of the sodium ethyl salts of acids containing respectively 1 and 6 CH_2 groups, by the interaction of the two different discharged anions.

It is somewhat surprising that the method of electrosynthesis, an outline of which has been indicated, is not more extensively used in practice. Doubtless this is in part due to the care which must be exercised in adjusting the physical conditions in order to secure a successful result. Concentration of solution, temperature, material of electrode, electromotive force and current density at the anode all play an important part in determining the result of the electrolysis. Thus, for example, if we substitute a gold anode for a platinum anode in the electrolysis of a solution of potassium acetate, although the gold electrode is not attacked, and all other conditions remain the same, we obtain at the anode oxygen instead of a mixture of ethane and carbon dioxide. Here synthesis has not been effected: instead of the reaction of equation (4) it is now the action analogous to equation (3) which preponderates.

It is perhaps noteworthy that unlike many of the ordinary synthetic methods of organic chemistry which only succeed in unusual solvents or at comparatively high temperatures, the method of electrosynthesis yields the most successful results in aqueous solution and at the ordinary temperatures, resembling therein the synthetic processes which occur in plants and animals.

[J. W.]

WEEKLY EVENING MEETING,

Friday, April 29, 1921.

The Hon Sir Charles A. Parsons, K.C.B. J.P. M.A. D.Sc. LL.D. F.R.S. M.R.I., in the Chair.

Sir Frank Watson Dyson, LL.D. F.R.S.,
The Astronomer Royal.

Advances in Astronomy.

In the past ten years a number of the large telescopes of the world have been applied to the determination of stellar parallax. The principle of the method is well known and is extremely simple, merely consisting in the detection of the small annual movement of a near star with reference to more distant stars caused by the different position occupied by the observer in consequence of the earth's annual revolution round the sun. The whole difficulty consists in the extreme minutenes of the angle to be measured. If two railway lines, starting at King's Cross, instead of remaining parallel, met at Newcastle the angle between them would be of the order of the angle to be measured in finding the distances of the nearest stars. To form an idea of what is now being done by large telescopes using photographic methods, imagine two plumb-lines 5 ft. apart. They are sensibly parallel, but actually meet at the centre of the earth, and the angle between them is $0 \cdot 05''$. An angle of this size is measured with an accuracy of $\pm 0 \cdot 01''$. Results of this high value were first obtained by Prof. Schlesinger at the Yerkes Observatory. At the present time the observatories of Allegheny, Greenwich, McCormick, Mount Wilson, Yerkes, and a number of others are engaged on a comprehensive programme. At Greenwich we determine the parallaxes of fifty stars a year; at some of the American observatories many more.

Necessarily, a good deal of care is required both in taking the photographs and in measuring them. The image of a star may have a diameter of $2''$ or $3''$, and the position of its centre should be measurable to between $\frac{1}{50}$th and $\frac{1}{100}$th of this amount. The methods of measurement present some points of interest which need not be described now, but a word or two about the precautions to be observed in taking the photographs may be of interest. The images must be as circular and uniform as possible. (1) The guiding of the telescope

must be as perfect as possible. (2) The lenses of large object-glasses must be adjusted with great care so that there may be neither tilt nor eccentricity between them. (3) Photographs should all be taken with the telescope pointing in the same direction. One cannot be taken when the field is east and another when it is west. Atmospheric dispersion and possibly minute flexure of the lenses cause slight deformation of the images which may be scarcely visible to the eye, but appear in measures. (4) The star the parallax of which is being determined and the comparison stars should have approximately equal images on the photograph. This is secured by means of a rotating shutter, a neutral screen, or the use of a grating in front of the objective.

The purpose of (3) and (4) is to make any residual errors the same for the parallax star and the comparison stars, and so far as possible the same on all photographs.

The knowledge of the distance of a star gives us immediately its luminosity or the amount of light it emits as compared with the sun. There is a very great range in luminosity even for stars of the same spectral type. Now the stars have been arranged in an order according to the spectra, which agrees fairly well with their order in colour from blue to red, and is essentially an arrangement according to temperature. This may be regarded as an extremely good first approximation to a classification of stellar spectra. But it does not detect any differences attributable to absolute luminosity, though presumably density and gravity at the surface layer of the star from which the lines in the spectrum have their origin must be widely different.

A few years ago a very fruitful investigation was commenced at Mount Wilson by Adams and Kohlschüter. By a close comparison of the spectra of stars of the same spectral class, but differing greatly in absolute luminosity, they detected lines the intensities of which differ. Adams and his coadjutors at Mount Wilson have pursued this research with very great success. They have found in stellar spectra a number of pairs of neighbouring lines, one line of each pair being independent of the absolute luminosity, while the other changes in intensity with the luminosity of the star. They have measured the relative intensities of these pairs of lines, and compared their measures with the luminosities of 650 stars already known through the trigonometrical determinations of parallax made at Allegheny, McCormick, Mount Wilson, and Yerkes. Thus they have found the luminosities of stars corresponding to different intensities of the lines. They have recently published a catalogue (*Astrophysical Journal*, Jan., 1921, Vol. LIII. p. 13) giving the luminosities and parallaxes of 1680 stars.

The advantage of this method is that it extends the range of parallax determinations beyond the limit (say) 0·02″ of the trigonometrical method, the limit of the spectroscopic method being

determined only by the capacity of large telescopes to give measurable spectra. In the table a comparison is given with unpublished results at Greenwich obtained by the trigonometrical method :—

No.	App. mag.	Mag. at 10 parsecs	Parallax	
			Mount Wilson	Greenwich
	m.	m.	″	″
B 1673	5·6	4·2	0·052	0·034
B 2897	6·1	4·3	0·044	0·040
B 2971	7·8	7·2	0·076	0·088
C 1604	8·2	4·9	0·022	0·015
B 3983	6·9	5·7	0·058	0·052
B 4181	5·0	1·7	0·022	0·041
B 4234	6·4	2·4	0·016	0·013
C 2242	7·6	5·4	0·036	0·046
B 4322	4·8	3·6	0·058	0·031
B 5009	4·8	3·8	0·158	0·171
B 6129	6·6	6·7	0·105	0·076

Comparison of these results, obtained by entirely different methods, shows the accuracy of 20 per cent. claimed for Mount Wilson, and $\pm 0 \cdot 010''$ for Greenwich is reached.

A third method which is employed extensively for determining stellar distances depends on the fact that the masses of stars lie within very restricted limits. It is applicable only to double stars, and depends on Kepler's third law, $M + m = a^3/P^2$, where M, m are the masses, a is the mean distance between the components, and P the period of a double star. When P is known and $M + m$ assumed, a is found, and, further, as the cube root of $M + m$ is involved, an error in the assumed mass produces a much smaller error in the mean distance. Now the *angular* mean distance is determined by direct observation for all double stars the orbits of which can be calculated. At the present time this amounts to more than 150. But it has been shown by Hertzsprung and Russell that for double stars which have completed too small a portion of their orbits for their periods to be known it is still possible to obtain their "hypothetical" parallax with considerable probability. The method has been recently applied at Greenwich to obtain the parallaxes of a large number of stars, and the accordance with the results found by the trigonometrical and spectroscopic methods is very satisfactory (see a paper in Monthly Notices R.A.S., November, 1920, Vol. LXXXI. p. 2, by Messrs. Jackson and Furner).

I believe there is in preparation by American astronomers a catalogue giving the parallaxes of 3000 stars, about half of which have been determined by two at least of these three methods. We may expect that in the course of a very few years the distances of all stars visible to the naked eye in the northern hemisphere will have

been determined, as well as those of many fainter stars. This great accession of knowledge of stellar distances carries with it a corresponding increase with reference to the luminosities, sizes, masses, densities, and velocities of stars of different spectral classes.

[F. W. D.]

ANNUAL MEETING,

Monday, May 2, 1921.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer and Vice-President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1920, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Fifty-four new Members were elected in 1920.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1920.

The Books and Pamphlets presented in 1920 amounted to 163 volumes, making, with 465 volumes (including Periodicals bound) purchased by the Managers, a total of 628 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :

PRESIDENT—The Duke of Northumberland, M.V.O. C.B.E.
TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
SECRETARY—Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S.

MANAGERS.

The Rt. Hon. Lord Blyth, J.P.
Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. D.L. J.P. LL.D.
John Mitchell Bruce, C.V.O. M.D. LL.D.
Sir James J. Dobbie, LL.D. D.Sc. F.R.S.
Percy Faraday Frankland, C.B.E. LL.D. Sc.D. F.R.S.
Alexander H. Goschen.
The Rt. Hon. Earl Iveagh, K.P. G.C.V.O. LL.D. F.R.S.
H. R. Kempe, M.Inst.C.E.
Sir Ernest Moon, K.C.B. K.C. LL.B.
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Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
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Sir Alfred Yarrow, Bart., M.Inst.C.E.
The Rt. Hon. Sir Robert Younger, G.B.E.

VISITORS.

James Henly Batty.
Alfred Carpmael, B.A.
Frank Clowes, D.Sc., F.C.S.
Edward Dent, M.A.
Lieut.-Col. Henry E. Gaulter, F.R.G.S.
George H. Griffin.
W. E. Lawson Johnston.
John R. Leeson, J.P. M.D. C.M.
F. K. McClean, F.R.A.S.
Edward Montefiore Micholls, M.A.
Hugh Munro Ross, B.A.
Joseph Shaw, K.C.
Sidney Skinner, M.A.
Thomas H. Sowerby, B.A.
William Stone, M.A. F.L.S.

WEEKLY EVENING MEETING,

Friday, May 6, 1921.

SIR JAMES CRICHTON BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

SIR ROBERT ROBERTSON, K.B.E. D.Sc. F.R.S. M.R.I.

War Development of Explosives.

IT is not proposed to describe the great factories that arose during the war for the manufacture of explosives, but to indicate by one or two examples some of the conditions which led to developments.

PRODUCTION.

The enormous weekly production was reached of 1500 tons of trinitrotoluene, 300 tons of picric acid, 3000 tons of ammonium nitrate, and 2000 tons of cordite. To produce these were required such weekly quantities as the following: 6600 tons of pyrites, or 2700 tons of sulphur, 8300 tons of Chile saltpetre, 720 tons of toluene (from 600,000 tons of coal), 162 tons of phenol (which would have required 1,000,000 tons of coal, if synthetic production had not been established), 700 tons of ammonia (from 250,000 tons of coal), 374 tons of glycerine (from 2700 tons of fat), 700 tons of cotton cellulose (from 1060 tons of wastes), and 1200 tons of alcohol and ether (from 4200 tons of grain).

These numbers indicate not only the magnitude of the production, but also the interdependence of a large number of industrial chemical activities, and, although many of the products were derived from our own coal, it brings home the dependence of the country on overseas transport of many of the essential substances, such as pyrites, sulphur, Chile nitrate, and cotton.

FIRING AND DETONATION OF A SHELL.

The Propellant.—The processes for the manufacture of cordite and of its ingredients had been the subject of study, and considerable advances had been made, so that it might fairly be claimed that this

country led the way in the technique and safety precautions involved in the manufacture of propellants. The existing factories were also capable of extension, until the demand became so great that additional ones had to be erected.

At first, the propellant used was cordite M.D., composed of nitroglycerine, guncotton, and mineral jelly, in which acetone was used to gelatinise the guncotton. A nitrocellulose powder obtained from America was also used. The demamd for propellant to be made in this country ultimately reached 1500 tons a week, and this, even with an efficient system of acetone recovery, would have involved an expenditure of that solvent of about 400 tons a week. On account of the shortage of supply of this solvent, a new propellant for the Land Service was introduced—cordite R.D.B.—in which ether-alcohol was substituted for acetone as a solvent, a change necessitating the choice of a nitrocellulose of a lower degree of nitration than guncotton, and alterations in the proportions of the other ingredients. For the new propellant the conditions were laid down, and met, that it should have the same heat energy, that it should give the same ballistics as cordite M.D., in order to avoid alteration in calculating ranges from data obtained with the older propellant, and that it should be capable of being manufactured by the machinery available and with the technique of manufacture known in the country.

The main changes introduced were in the manufacture of the nitrocellulose and in the supply of the solvent. As ether-alcohol is a less powerful solvent than acetone, even for the special nitrocellulose employed, a strict definition of the nitrocellulose was necessary, and the necessity to provide this in suitable form led to much investigative work on the nature of the cellulose, with the result that its manufacture was brought under a system of strict chemical control. This control had among its objects the elimination of ligneous impurities and the standardisation of the viscosity of the cellulose, since if its viscosity was uniform and low, it was found that the gelatinisation of the nitrocellulose when incorporated with the nitroglycerine and mineral jelly was greatly facilitated, and the production of uniform cords assisted. Ligneous matter in the cellulose was rendered visible by a process in which the woody matter was dyed selectively, and the viscosity of the cellulose was measured by the rate of fall of a steel sphere falling through a solution of cellulose.

The supply of alcohol was obtained entirely from the distilleries of this country, and a large plant for converting a portion of it into ether was erected at Gretna. Nearly 1000 tons of alcohol, or the equivalent of about 200,000 gallons of proof spirit, were required for the production of the 1500 tons of R.D.B. cordite a week, and it was this requirement which led to the restricted sale and increased cost of whisky.

THE LAND SERVICE HIGH EXPLOSIVE SHELL.

The problem of bringing trinitrotoluene to complete detonation with certainty had been worked out some years before the war. The type of shell to be described was used by the Land Service, and contains the results of developments mostly made during the war.

Prior to the war the Land Service used for the most part shrapnel shell, designed to project a shower of lead bullets, efficacious against *personnel*, but of little value in attacking fortified positions, for which high explosive shell is required.

Shrapnel was very largely used by the Land Service throughout the war, but the earlier type of high explosive shell filled with lyddite (picric acid), and brought to explosion by the ignition of a fiercely burning mixture, was abandoned for one in which true detonation was secured with certainty. The latest type of high explosive shell was exemplified by a 4·5 in. howitzer shell fitted with a graze fuze (Fig. 1).

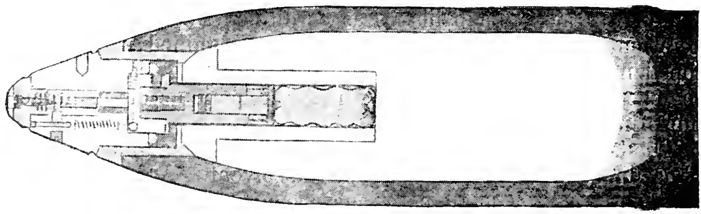

FIG. 1.

The Fuze.—A graze fuze is a mechanism which gives rise to a flash when the shell grazes on the ground. It must be capable of being handled roughly without firing, and must not act when the considerable forces involved in firing it from a gun are impressed upon it and upon all its parts. The magnitude of these forces is illustrated by the fact that a fuze weighing $2\frac{1}{2}$ lb. when fired from an eighteen-pounder gun weighs about 11 tons—the stress corresponding to 15,000 times the acceleration due to gravity. These forces are taken advantage of to render the fuze "live"—that is, to put it into a condition when it will act on the slightest provocation.

In the interior of the fuze is a brass cylinder with an axial hole, on the top of which is placed a capsule containing a highly sensitive flash composition. To prevent this cylinder from moving forward in handling, a bolt lies athwart its top edge, and this bolt is retained in this position by a small pin placed vertically at the back of the bolt and having its base pressed upward by a spring working in a vertical cylindrical cavity. On firing, this pin, weighing 1·3 grams, is acted on by a force equivalent to 20 kg., overcomes the resistance

of its spring, and recedes into its cavity. The force due to the shell's rotation causes the bolt to fly outwards, thus freeing the brass cylinder, which is now prevented from moving forward on to a needle only by the interposition of a light spring. The fuze is now "live," and on the slightest check being given to the forward movement of the shell, as, for example, by grazing on soft earth, the cylinder moves forward by its own inertia on to the needle, which pricks the capsule, causing a jet of flame to pass down the centre of the fuze. The object of all this mechanism is to supply at the proper time a flash for operating the next member, the gaine, where it gives rise to a detonation.

The Gaine.—This is a tube (from French *gaine*, a sheath) with steel walls of quarter-inch annulus. In its upper portion is a pellet of gunpowder which is ignited by the flash from the fuze, and sends a larger flash on to an open capsule containing fulminate of mercury situated over pellets of tetryl. The fulminate detonates, and in turn causes the tetryl to detonate, and to deliver from the bottom end of the gaine a very intense blow to a series of explosive intermediaries which communicate the detonation to the main bursting charge.

Intermediaries.—The first of these is a bag of T.N.T. crystals situated in a thin steel container tube which encloses it and the gaine. This T.N.T., on detonation, brings to detonation an annular layer of T.N.T. cast round the container, and this in turn brings about the detonation of the main charge of the shell. The train of detonation is thus somewhat complicated, and in its evolution many important principles had to be observed.

Sensitiveness and Violence.—Thus the sensitiveness of the various explosives used had to be determined, since, on account of the magnitude of the acceleration imparted to all parts of the shell on firing it from a gun, a column of a sensitive explosive over a certain length and weight will be liable to detonate on account of the sudden force applied. In proportion to their sensitiveness to mechanical shock, therefore, explosives in shell must be graduated in regard to length of column employed. A general principle is to have next to the detonator a somewhat sensitive explosive, and to reinforce the impulse derived from it by one less sensitive, but still delivering an intense blow. It is important, therefore, to have quantitative values for the sensitiveness of explosives to mechanical shock, and some of the values thus obtained are given in the following table :—

	Figure of Insensitiveness (Picric acid = 100)
Mercury fulminate	10
Nitroglycerine	13
Dry guncotton	23
Tetryl	70
Tetranitroaniline	86
Picric acid	100
Trinitrotoluene	115
Amatol 80/20	120

It is important also to know the violence of the various explosives used, both by themselves and also when assembled in the various components, and it was in this connection that the principle of the pressure bar, enunciated by the late Prof. Bertram Hopkinson in a discourse to the Royal Institution in January of 1912,* was of the greatest value. This depends on the experimental resolution of the momentum of the blow into pressure and time. When a charge is fired against the end of a cylindrical steel bar ballistically suspended, a wave of compression travels along the bar and is reflected at the far end as a wave of tension. To investigate the properties of the wave, a short length of the end of the bar farthest from the end to which the blow is delivered is cut off and the faces are surfaced, the short piece (known as the time-piece) being caused to adhere closely to the bar, usually by a film of vaseline. The compression wave travels unchanged through the joint into the time-piece, but the reflected tension cannot pass through it. Hence when the amplitude of the reflected tension wave reaching the joint becomes greater than that of the oncoming compression wave, the time-piece is projected from the shaft with a momentum which depends on the pressure exerted by the explosive and the time taken by the wave to traverse the length of the time-piece. This momentum is measured by catching the time-piece in a ballistic pendulum, and, the velocity of the propagation of the wave through steel being known, the mean pressure exerted during an extremely small time interval can be calculated.

[One of the instruments for determining the pressure developed by a detonator was shown, and a detonator fired, the mark drawn by the swing of the pendulum which caught the time-piece being shown on the screen.]

The application of this apparatus not only gave important information as to the limiting quantity of fulminate necessary to bring about complete detonation of the tetryl and as to the effect of the thickness of the wall of the gaine, but it also emphasized the necessity for avoiding gaps in the train of detonation on account of the very rapid falling off in violence of the blow when even a small air-gap is introduced.

Main Filling.—It was early recognised that the supply of picric acid and T.N.T. by itself would be quite insufficient. It was at this point that the late Lord Moulton took steps to secure supplies of essential explosives and their ingredients, with such success that the supply of explosives shortly came to be ahead of the demand. But even when a method for the production of T.N.T. had been worked out, and its supply on a fairly large scale was in prospect, it was apparent that the demand for high explosives was such that it could not be met by the supplies of nitro-compounds in sight.

Experiments were then made to test the capabilities of mixtures of ammonium nitrate and trinitrotoluene for shell filling, and these

* Proc. Royal Inst., Vol. XX. p. 275.

gave much promise from the start. They were found to possess the requisite degree of inertness and insensitiveness to enable them to withstand set-back on firing from a gun, to have a high rate of detonation, and when detonated in a shell, as was done first in March 1915, to give evidence of the required violence necessary to fragment the shell.

The first mixture (later termed amatol 40/60, these being the proportions of ammonium nitrate to T.N.T.) was capable of being poured as a thick porridge into shell, and so presented few difficulties for large-scale production. This was at once followed up by similar experiments with a still greater proportion of ammonium nitrate, up to that which is practically the theoretical one for complete combustion of all the carbon of the trinitrotoluene to carbon dioxide, and of all the hydrogen in both substances to water. This explosive, amatol 80/20, was fired in a shell in April 1915, and gave excellent results. Its explosive properties, as regards insensitiveness, stability, and tests for power, were satisfactory, and it was almost immediately approved as a Service explosive.

Amatol 80/20.—The development of amatol 80/20 was slower. Prepared originally on the large scale by bringing together the finely powdered ingredients in a mixing machine, or by grinding them under edge-runners, 80/20 amatol was ultimately most readily produced by taking advantage of the plasticity of the heated mixture due to the trinitrotoluene melting. Hydraulic presses were used for introducing the powdered or ground explosive into shell; for the plastic 80/20, a worm feed was found expeditious and rapid.

In the course of the manufacture of the enormous quantities of these substances many points of interest and of difficulty arose, which were solved by the assistance of more and more scientific investigators.

The following tables give some data on the explosive properties of the amatols in comparison with some other explosives :—

Heat of Detonation and Gases Evolved.

	Calories per gram (water gaseous)	Total gases c.c. per gram
Picric acid	914	744
Trinitrotoluene . . .	924	728
Amatol 40/60 . . .	920	892
Amatol 80/20 . . .	1004	907
Tetryl	1090	794
Guncotton	892	875
Nitroglycerine . . .	1478	713

Rates of Detonation.

	Density of loading	Metres per second
Nitroglycerine . . .	(Liquid)	8000
Tetryl	1·63	7520
Guncotton (dry) . . .	1·20	7300
Picric acid	1·63	7250
Trinitrotoluene . . .	1·57	6950
Amatol 40/60 . . .	1·55	6470
Amatol 80/20 . . .	1·50	5080

PRESSURES DEVELOPED BY AMMONIUM NITRATE, AMATOLS, AND T.N.T.

Ammonium Nitrate	Trinitrotoluene	Tons per sq. in. in $0{\cdot}5 \times 10^{-5}$ sec.
100	0	12·5
99·5	0·5	15·2
99	1	18·3
98	2	20·0
95	5	25·2
90	10	30·5
80	20	38·1
40 (at density 1·55)	60	53·9
0 (at density 1·55)	100	55·0

It will be seen that the addition of 40 per cent. of ammonium nitrate to T.N.T. does not markedly reduce its heat value, rate of detonation, or pressure developed, and that 80/20 has a high content of heat energy, but a rate of detonation and pressure lower than T.N.T. itself. It is, however, still sufficiently violent to fragment shell satisfactorily, and the somewhat slower development of the pressure, together with the high calorific value of the explosive, may be of advantage in enabling the fragments to acquire a higher velocity. It will also be observed that ammonium nitrate itself under a powerful initial impulse gives rise to a notable pressure, so that that ingredient is not to be looked on as a diluent of the T.N.T., but as an explosive substance, as well as a purveyor of the oxygen in which T.N.T. is deficient.

Smoke.—For the purpose of correct ranging and locating the position of burst, an explosive developing smoke is desirable. Amatol 80/20, when used alone, had the disadvantage that it gave no smoke, as the products of the detonation are colourless gases, thus :—

$$2C_7H_3N_3O_6 + 21NH_4NO_3 = 24N_2 + 47H_2O + 14CO_2 ;$$

whereas, when picric acid or trinitrotoluene detonates, a large quantity of unconsumed carbon is set free, affording a black cloud useful for the purpose of observation.

Mixtures capable of producing a white smoke, useful for aerial observation, were then added, and as a result of investigations as to the best method of securing its dissociaton, ammonium chloride in conjunction with the ingredients of amatol was localised at the base of the filling.

Needless to say, there were many other developments in explosives practice during the war, but the example of the train of detonation leading up to the complete detonation of a high explosive shell was chosen to exemplify the subject of this discourse, since it included many features and new problems which had an intimate connection with the technical development of the subject.

To secure the high percentage of detonations that our artillerists obtained with the freedom from prematures which they always

demanded, it was necessary to have each part of the somewhat complicated train as nearly perfect as possible not only in design, in order to withstand the effects of rough usage and of set-back in the gun, but also in workmanship, both mechanical and chemical as to purity of materials. This was achieved by the co-ordination of a large number of industries organised on a scientific basis, and these were becoming every day more and more efficient. War is now so highly organised that for its successful prosecution all the technical industry of the country is brought under requisition, and to succeed requires a higher development in research, applied methods, and industrial progress than belongs to the enemy.

The effort made by this country in the time of stress to overcome deficiencies in these respects was successful as a great technical achievement, and should be an encouragement to us to look forward to an equal development of our scientific industries under the stress of a competitive peace.

[R. R.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Wilfred E. Watson Baker, F.R.M.S.
Reginald Langdon Downe, M.A. M.B.
Robert William Paul, M.I.E.E.
Arthur S. Tabor, M.A.
Mrs. S. S. Williams

were elected Members.

The Chairman announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

Horace T. Brown, LL.D. F.R.S.
J. H. Balfour Browne, K.C. D.L. J.P. LL.D.
Sir James J. Dobbie, LL.D. D.Sc. F.R.S.
The Right Hon. Earl Iveagh, K.P. G.C.V.O. LL.D. F.R.S.
Sir Ernest Moon, K.C.B. K.C. LL.B.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S. (Treasurer)
Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S. (Secretary)

The Chairman read the following letter from Dr. G. E. Hale, acknowledging the award of the Actonian Prize :—

CARNEGIE INSTITUTION OF WASHINGTON,
MOUNT WILSON SOLAR OBSERVATORY,
PASADENA, CALIFORNIA.
March 8th, 1921.

Henry Young, Esq.,
Assistant Secretary, Royal Institution,
21 Albemarle Street, London, W.1.

DEAR MR. YOUNG,

I beg to thank you for your kind letter of February 11, informing me of the award of the Actonian Prize for my contributions to Solar Physics and Stellar Evolution. I have also received from Messrs. Drummond a draft for One Hundred Guineas, receipt for which is enclosed.

On the arrival of the marconigram which gave me the first news of the award, I at once cabled my thanks to the Managers. I trust you will inform them how sincerely I appreciate the great and unexpected honour they have conferred upon me. For many years I have been an ardent admirer of the

Royal Institution, to which I return on every possible occasion. In some mysterious way, which the conservators of other scientific establishments must envy, the Managers and the Professor of Chemistry have succeeded in retaining and rendering tangible the very atmosphere of research bequeathed to them by Rumford, Young, Davy and Faraday. In no other Laboratory, however rich in tradition, have I felt so potently the direct inspiration of the masters of the past. The discernment with which their successors have been chosen, and the notable and unbroken progress of the laboratories and lectures, are doubtless the chief factors in this achievement, which must have impressed the thousands who have shared in the privileges of the Institution.

Under the circumstances, the Managers will understand why I take very exceptional satisfaction in their award of the Actonian Prize. But in simple fairness to my associates in research at the Kenwood, Yerkes, and Mount Wilson Observatories, I beg permission to point out that a large part of the credit for the work in question belongs to them, because of the important part they have had in it. In order to give tangible expression to my indebtedness, I have requested a Committee to consider and report to me on the best way of expending the sum of One Hundred Guineas for the advantage of the members of the Mount Wilson Observatory, who have been most closely connected with the researches referred to by the Managers. I hope this may be accomplished in such a way as to assist in perpetuating here some of the admirable traditions of the Royal Institution.

Believe me,
Yours faithfully,
(Signed) George E. Hale.

Sir Joseph John Thomson, O.M. M.A. LL.D. D.Sc. F.R.S., was elected Honorary Professor of Natural Philosophy.

Sir Ernest Rutherford, D.Sc. LL.D. F.R.S., was elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, May 13, 1921.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

William Bateson, M.A. F.R.S.

The Determination of Sex.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, May 20, 1921.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Manager and Vice-President, in the Chair.

PROFESSOR E. H. STARLING, C.M.G. M.D. D.Sc. F.R.C.P. F.R.S.

The Law of the Heart.

THE discovery by Harvey of the circulation of the blood, and of the part played by the heart in carrying on this circulation, is one of the few scientific discoveries which have become common knowledge. We have to think of the body as a collection of mechanisms or machines, each one of which is doing some form of work for one common end—i.e. the preservation of the body. For this work the oxidation of the food taken in at intervals during the day provides the energy; thus each part of the body must be supplied not only with food derived from the alimentary canal, but also with the oxygen taken in with the air we breathe into the lungs. Like any other machine, each body mechanism produces, as a result of this consumption of the food, waste gases and other waste products which have to be carried to the lungs or to the kidneys and there cleared out of the body. It is for this reason that the existence of the higher animal demands a common fluid, the blood, which can carry food, oxygen or carbonic acid, and which is maintained in continual circulation between all the organs of the body, so that the alimentary canal, for instance, may serve for the maintenance of all parts, and the lungs can supply oxygen to these parts or excrete the carbonic acid which is produced as a result of their activity.

But a uniform mechanical circulation would be of little value to the body, since the activities of all its parts vary within wide limits. Thus, during muscular exercise the activity of the muscles may be increased ten-fold or more, and this increase means a corresponding augmentation in their call for oxygen and in the quantity of waste products, especially carbonic acid, that they produce. Since the oxygen is carried by the blood, it follows that for the continued functioning of the muscles these must receive a blood supply which is ten times greater during activity than during rest, if their activity is not to be brought to an end by a species of suffocation. We thus

see that in any violent exercise involving the greater number of the muscles of the body, the circulation must be increased in force seven to ten times, and the heart, which is the pump maintaining the circulation, must under these conditions do from seven to ten times as much work as during rest.

The Mechanism of Adaptation.

What is the mechanism of these adaptations? How is it that the heart is able to carry on a circulation which may vary from a passage of 3 litres of blood per minute up to 30 litres of blood per minute? (these figures representing the extreme limits between which the output of the heart-pump may vary according to the condition of the body). It might be thought that we are dealing here simply with the influence of the central nervous system, which adapts the activity of the muscles of the body to the requirements of the environment, and that the heart being a muscle would be stimulated to contract more strongly at the same time as the nervous system calls into activity the voluntary muscles of the body. There is no doubt that the heart is under the control of the central nervous system, so that its action can be altered, increased or diminished by the brain in accordance with the needs of the economy, but in the heart we find also a wonderful power of adaptation to the varying requirements of the organism which is quite independent of the central nervous system.

This can be shown quite easily either in the cold-blooded or warm-blooded animal. The heart of the frog and tortoise can be cut out and will continue beating for hours or even days. It has long been known that the heart of the mammal would beat for some minutes after being cut out of the body, but if we take pains to ensure that the muscles constituting the walls of the heart continue to receive their supply of oxygenated blood, the mammalian heart can be made to beat for 8 to 12 hours after the death of the animal from which it is taken. In order to investigate this properly, we want to make such a preparation that we can control at will all the conditions which may affect the action of the heart—viz. the amount of blood flowing into the heart from the big veins, the resistance which the heart has to overcome when it drives the blood out into the arteries, and the temperature at which the heart contracts. We must be able to measure at any time the output of the heart, the arterial pressure it maintains, its changes in volume during contraction, the pressure in all its cavities during contraction, the amount of blood flowing through the blood vessels of its walls, and its chemical exchanges, as measured by the amount of oxygen which it takes up and the amount of carbonic acid which it produces. It is these chemical changes which give the energy for the work of the heart.

The Heart-Lung Preparation.

All these procedures and controls can be carried out in the heart-lung preparation. In this preparation the pulmonary circulation from right ventricle to left auricle is left intact, and by means of artificial respiration the lungs are blown up rhythmically so that the blood in its course may take up oxygen and get rid of carbonic acid. The whole systemic circulation is replaced by rubber tubes. A glass tube is tied into the largest branch of the aorta, all the other branches being tied, so that the blood driven out by the left ventricle can only escape by the glass tube. From the glass tube a rubber tube passes to a thin rubber tube contained within a wide glass tube. This thin rubber tube can be compressed to any desired extent by pumping air at a known pressure into the glass tube surrounding it. We can thus vary at pleasure the resistance which has to be overcome by the left ventricle, and, by maintaining a normal pressure in the beginning of the aorta, ensure a proper supply of oxygenated blood through the coronary arteries to the muscular tissue of the ventricles. It is this fact which makes it possible for the warm-blooded heart to continue to beat for eight to twelve hours after removal from the body. On the other side of the artificial resistance the blood is led through a spiral immersed in warm water to keep the blood at body temperature, and then passes into a reservoir from which a wide rubber tube leads to a glass tube placed in the big vein opening into the right auricle. By means of a screw clip on this tube the inflow of blood may be regulated to any desired extent, and can be kept constant while other conditions are varied. Thus in this preparation the three chief factors, temperature, the inflow of blood, and the resistance to the outflow of blood, can be varied separately and at the will of the operator. Any of the heart cavities or any part of the circuit can be connected to manometers so as to record the pressure of the fluid, and by means of a side tube placed just beyond the artificial resistance we can allow the blood to flow off into a graduated cylinder, and thus measure the time taken by the left ventricle to expel 50 or 100 cc. of blood, thus measuring the average output of the organ.

An Experiment Described.

A typical experiment may be divided into six stages. Records of one experiment show that in the first stage the heart was beating at a normal rate (72 per minute), the blood pressure varied from 100 mm. Hg., and the output of the heart was 240 cc. of blood per minute. In the second stage the resistance to the flow of blood through the tubes was increased to such an extent that the pressure rose to 160 to 180 mm. Hg. The heart continued to beat, and for a time put out just as much blood as it did at the lower pressure. In

the third stage the artificial resistance was suddenly reduced to zero, the arterial pressure fell to about 20 mm. Hg., but the heart beat regularly and the outflow of blood was unaltered because the inflow of blood had not been altered. In the fourth stage the inflow of blood was raised suddenly to 600 cc. per minute. The heart became bigger, but the regularity of its contractions remained unaltered, and it drove forward all the blood that it received.

The same thing happened in the fifth stage, in which the artificial resistance was raised simultaneously with the venous inflow. The reason for these phenomena is that within certain limits the heart isolated from the body can respond to all the demands made upon it; it can overcome a higher resistance, and it can pump out more fluid. In the sixth stage the inflow of blood was further increased to 1200 cc. per minute, and the artificial resistance was increased until the blood pressure rose to 200 mm. Hg. This was too much for the heart, which began to beat irregularly and dilate widely. It would have failed altogether if the pressure surrounding the thin rubber tube had not then been released, to allow the artificial pressure to drop to a level at which the left ventricle could empty itself.

If during this experiment the amount of oxygen taken up by the blood had been measured, and also the amount of carbonic acid given off by this fluid in passing through the lungs, an increase in both these amounts would have been found during the stage at which greater demands were being made on the heart. That is to say, the greater the work done by the heart, the greater the chemical changes to supply energy. A motor-car may be running steadily with an even beat of its engines along a level road; when it comes to a hill it will slow up and finally stop unless the chauffeur increases the chemical changes and the energy of each explosion within the cylinder by opening the throttle and letting in more mixture of petrol and air. In the case of the heart there is no chauffeur, but there is some automatic regulation by which the heart increases its chemical changes, and therefore the energy of each beat, in exact proportion to the work which is demanded of it. It is the nature of this automatic regulation which concerns us now.

The Nature of the Automatic Regulation of the Heart.

By a careful observation of the changes in the heart in the experiment described above we may arrive at some clue to the nature of the pressure, but more accurate methods are necessary if we are to be certain of the correctness of our guess. We must under these varying conditions, measure: (1) the pressure in the heart cavities produced at each contraction; (2) the volume of the heart cavities—i.e. the length of the muscle fibres of their walls. The first we measured in the experiment described by connecting the interior of each cavity in turn with a quickly acting manometer, the excursions

of which are registered by an optical method so as to avoid the instrumental vibrations of a lever. The curve of pressure obtained under two conditions—i.e. low and high artificial resistance—could then be plotted. It must be remembered that the heart was sending on in each case all the blood that it received, though the work necessary under the high pressure was two or three times as great as that necessary to send on the blood at the low pressure. To measure the volume of the heart the ventricles are enclosed in an instrument known as a cardiometer. This communicates with a piston recorder so that the change of the volume of the ventricles at each beat can be registered on a moving surface.

The question we have to decide is, How does the heart know when it is relaxed that at the next contraction it will have to exert more force than it did previously, when the arterial resistance to be overcome was lower? If we measure the pressure in the ventricles in the manner just described we find that during the period of relaxation of the ventricles, the pressure in its cavities is approximately zero, whether the artificial pressure which it has to overcome at its next beat is 50 or 150 mm. Hg. It is not, therefore, the tension on the walls of the heart which determines the strength of its contraction at its next beat. When, however, we come to measure the volume of the heart, we find that in the isolated heart this is directly proportioned to the work which the heart has to accomplish. Thus we find that the larger the heart—i.e. the more it is dilated during diastole—the greater is the pressure that it will get up at the succeeding contraction or systole.

We may put this in another form, as is shown by continuing our experiment over several hours, when we find that the worse the condition of the heart muscle, the more it must dilate in order to get up an adequate pressure. Other things remaining equal, we thus see that the volume of the heart during diastole is a measure of its physiological condition, and we are not surprised that a failing heart means a dilated heart. Of course there is a limit to this power of adaptation. As the heart dilates it is working at an ever-increasing mechanical disadvantage, and a point will finally arrive at which this disadvantage more than counterbalances the physiological effect of dilatation. The heart then dilates widely and fails to empty its contents. Dilatation of the heart means elongation of the muscular fibres composing its walls, so that we may put the law of the heart another way and say that the longer its muscle fibres the greater is the energy developed at each contraction. But in this form this wonderful power of adaptation possessed by the heart becomes part of the general properties of all muscular tissues, since the same rule applies to the fibres composing our voluntary muscles. Can we obtain any more precise and physiological conception of what is involved in this relationship between length of fibre and strength of contraction? Microscopic examination of the fibres, either of the

heart or of voluntary muscle, shows that these are composed of innumerable fibrils, so that internally the muscle is made up of structures presenting an enormous extension of longitudinal surfaces. The more the muscle is stretched, the greater will be the extent of these surfaces. A large amount of evidence, based on the electrical and chemical changes occurring in muscle as a result of excitation, points to the contraction as being essentially a surface phenomenon—a molecular change over the whole of the longitudinal surface which may result in a polarisation or depolarisation of the surface and an increase of surface tension, so that the muscle is a surface tension machine in which there is on excitation a direct conversion of chemical into surface energy. The greater the surface the greater will be the number of molecules involved, so that increased length of muscle must increase at the same time the total chemical changes and the total tension produced by the summation of the surface tension of each fibril.

It is only by such a change of molecular dimensions that we can explain the rapidity of events in a muscle (the insect wing muscle can contract and relax 300 times per second), or the high efficiency of the machine, an efficiency which A. V. Hill has shown may amount to 100 per cent for each isolated contraction, and over a length of time to 50 per cent. As directly measured in the heart-lung preparation, we find a mechanical efficiency of about 25 to 30 per cent.

Conclusion.

It is impossible here to enter into the applications of this law of the heart, but so far it has not failed in accounting for the behaviour of this organ under all manner of conditions, either in health or disease. It is important to remember, however, that we are dealing here with the isolated heart. In the natural body the mechanisms which we have studied are fenced round, protected and aided by the complex activity of the central nervous system, which is always acting on the heart, balancing its activity against that of the blood vessels, and co-ordinating it with the events which are occurring in every other part of the body. All these factors must be taken into account when we are endeavouring to form a conception of the total behaviour of this organ under the varying activities of the intact animal.

[E. H. S.]

WEEKLY EVENING MEETING,

Friday, May 27, 1921.

COLONEL E. H. GROVE-HILLS, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

A. MALLOCK, F.R.S.

Elasticity.

BEFORE speaking of the more special problems relating to elasticity, on which I have been recently engaged, it may be as well to define the meaning of the word "elasticity" in its scientific sense.

In ordinary conversation the terms "elastic" and "elasticity" have many different interpretations, but in physics it denotes simply a property possessed by all matter, but in various degree—namely, that of returning to its original form and dimensions after any cause which produces the alterations has ceased to act.

It has nothing to do with the hardness, softness, brittleness, or flexibility of the material. These are terms which depend on the limits of elasticity, and not on the elasticity itself. A billiard ball, for instance, is not by any means soft, but it is exceedingly elastic, as is shown by the absence of any permanent mark at the point where it has received the impacts of other balls.

The measure of elasticity is given by the force required to produce some known alteration of form, not exceeding in amount that which will naturally disappear when the force is removed.

Elasticity is of two fundamentally distinct kinds. All isotropic matter (i.e. matter which has the same properties in all directions), whether solid, liquid or gaseous, may be compressed bodily without change of shape, and it may also be distorted without change of volume. The forces which resist these alterations are known respectively as volume elasticity, or compressibility, and rigidity.

Any possible change of shape can be brought about by a suitable combination of volume compression (or dilation) and distortion (or "shear" as it is often called, from the identity of the action of distortion with the effect produced by a pair of shears).

Liquids and gases oppose hardly any resistance to distortion; in fact the mathematical definition of a "perfect fluid" is a material which offers no resistance whatever to distortion. Matter, however, in whatever state it may exist, resists compression, and in most cases this resistance is very large. Water, for instance, though its rigidity is nearly negligible, is only compressed by one three-hundred-

thousandth part of its volume by a pressure of 1 lb. per square inch, and for solids, such as metals, the compression would be much less.

The resistance to compression of fluids is easily shown, but the corresponding resistance to dilation, though it exists, is more troublesome to demonstrate by experiment, owing to the difficulty of securing the adhesion of the fluid to the surface of the vessel which contains it.

Experiments have been shown in this room in which water and mercury, free from air and placed in perfectly clean vessels, have borne large negative pressures, and the increment of volume which can be brought about in this way is limited by the want of adherence to boundaries rather than by the want of coherence in the liquids themselves. There is, however, a limit to the coherence of all liquid and solid matter when the volume is forcibly increased, and this limit is reached when the expansion is a very small fraction of the original volume.

In gases, on the other hand, there is no coherence, and the particles instead of holding together naturally tend to separate, a fact which is well accounted for by what is known as the molecular theory.

According to this theory, all the molecules of which the gas is made up are in continual motion, generally striking and rebounding from one another or from the boundaries which enclose them after a short free path. Pressure in a gas depends on the effect of the sum of these continual impacts, and thus not only on the velocity of the molecules, but also on the total number of molecules in a definite volume—that is, on the density. Temperature, on the other hand, depends on the average velocity only, the average velocity being taken as the square root of the mean of the squares of the velocities of all the molecules engaged. For air at ordinary temperatures and at atmospheric pressures this velocity is somewhat in excess of 1,600 feet per second.

The relation between pressure, volume and temperature to which the molecular theory leads is :

$$\text{Pressure} \times \text{Volume} = \text{Absolute Temperature} \times \text{a Constant},$$

and this, taken as it stands, would indicate that if the temperatures were kept constant there would be no limit to the reduction of volume with the increase of pressure. The simple form of the theory takes no account of the size of the molecules themselves, and only represents the facts so long as the compression is insufficient to bring them into close contact with one another. When this contact does happen the variation of volume with pressure no longer depends on the reduction of the space between molecules, but on the compression of the actual molecules themselves, and there is every reason to believe that no force, however large, can reduce the volume of matter below a certain limit.

When the pressure is sufficiently increased and the temperature reduced, gases first liquefy and finally become solids, and so far as I know there have been no experimental determinations of the volume compressibility of liquid or solidified gases.

In fact singularly little is known as to the compressibility of any substance, even at the (what may be called) moderate pressure which can be produced in the laboratory. It is very difficult, for various reasons, to use pressure of 100 tons per square inch, which is very small compared to the pressure at the centre of the earth. By a well-known proposition it can be shown that if a certain mass is weighed at the earth's surface, and if the same mass were formed into a rod of uniform section and of the length of the earth's radius, and then lowered into a boring extending from the earth's surface to its centre, the gravitational attraction urging it downwards would be one-half of its surface weight.

The density of the earth is about 5·7, and the radius about 21 million feet. A rod one inch square, and of this density and length, would weigh just under 26 thousand tons at the earth's surface.

The pressure per square inch at the centre is therefore about 13,000 tons. At the centre of the sun the pressure would be more than thirty times as great.

Compared with this any experimental pressure seems insignificant.

The direct experiments on volume elasticity are chiefly on glass, water and mercury, in connection with the corrections for deep-sea thermometers, and also on certain other fluids, and the pressures employed range up to a few tons per square inch. (In Mr. C. A. Parsons' experiments on the compressibility of water the pressures were raised as high as 40 tons per square inch.)

I myself have made many trials of the compressibility of glass, indiarubber, various solid fats, and of minerals, such as asbestos, using pressures up to 24 tons. (These were in connection with the obturating pads used to make a gas-tight joint at the breech-blocks of large guns.) The method adopted in most of these experiments, including my own, depends on the measurement of the descent of a plunger under known loads into a strong vessel filled with the substance whose compressibility is to be found, and then applying corrections for the concomitant variation of the volume of the vessel, etc., induced by the applied pressure. It is in these corrections that the doubtful element appears in the results. For small pressures up to two tons per square inch I used a different procedure, in which the corrections were more easily dealt with. The matter for compression was placed in a glass cylinder, about ten inches high and three in diameter, the open end of which could be closed by a flat glass plate perforated with a small central hole.

A fine tube, connected with a small cistern of mercury, entered the vessel through this hole, and beneath it a second cup was

suspended to receive any mercury which might flow from the cistern. The matter to be tested was placed inside the cylinder, which was then completely filled with water, the cover was fitted on, and the whole placed in a strong steel explosion vessel also full of water, and the pressure was then pumped up as required. (Fig. 1.)

Thus there was practically no difference of pressure inside and outside the glass cylinder, and the volume of mercury which flowed through the tube (ascertained by weighing the contents of the cup) gave a measure of the volume compression of the contents of the former.

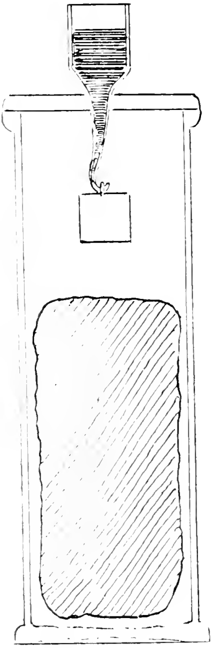

Fig. 1.

The only correction required is that for the volume compression of the glass and water, that for the small volume of mercury employed being negligible. The same method could probably be used with advantage at even the highest attainable pressures.

To sum up what is known as to the behaviour of matter when the volume pressure varies, it may be said that for any small change the variation of volume is directly proportional to the variations of pressure, and that while in the case of gases there is no limit to the possible expansion, no force, however large, can reduce the volume to less than the sum of the volumes of the molecules.

With liquids and solids there is a limit to the possible expansion, which is reached when the negative pressure exceeds the inter-molecular cohesion. When this limit is passed the extra space is either filled with the vapour of the substance, or is vacuous, and if the substance is a solid fracture occurs.

For a substance which has the same properties in all directions volume compression cannot cause rupture, and a cube, for instance, would remain a cube, no matter how much it was compressed, the only change being in the absolute dimension.

For crystals or other substances which have different compressibilities in different directions this is not so obvious, and it is not known whether the different elastic properties of crystals in the principal axes depend on compressibility or on rigidity, or both.

Rigidity, or resistance to change of shape, has been more fully investigated, experimentally at any rate, than compressibility.

What happens during distortion can be illustrated by supposing that two opposite faces of a cube are fixed to two parallel planes, and that these are forced to slide while remaining parallel and at a

constant distance from one another. It is evident that the volume of the cube is unchanged by this process, but the lengths of the diagonals of two of the faces are changed, one being shortened and the other lengthened.

If a square be inscribed on one of the faces before distortion with its edges parallel to the diagonals, the subsequent distortion changes it to a rectangle whose length is $1 + e$ and breadth $1 - e$. If F is the force required to produce the alteration, the rigidity is defined by the relation $F = ne$, or the coefficient of rigidity, n, is equal to F/e, and is the force which if applied as a pull over one pair of opposite faces of a cube and a push over another opposite pair would (if strain and stress remained directly proportioned to one another) double its length.

In ordinary solids strain and stress are only proportional within generally rather narrow limits, which, however, vary widely in different materials, as, for instance, in glass compared with india-rubber.

When these limits are exceeded the material is either ruptured or permanently distorted.

The point to which attention should be directed is that the limits for rigidity are quite different and apparently independent of those for dilatation.

As has been said before, volume elasticity and rigidity are the fundamental qualities which regulate the elastic behaviour of a solid, but the quality most ordinarily in evidence is the elastic resistance opposed to a direct pull.

This is known as Young's Modulus, and may be defined as the direct pull which would be required to double the length of a rod of uniform section, assuming strain and stress to be always proportional. If the modulus is denoted by E, and an extension e is caused by a direct pull F,

$$E = F/e.$$

E differs from the rigidity constant n in that no force is applied at right angles to the pull, and it involves the volume elasticity k, as may be shown by the diagram (practically the same as that given in Thomson and Tait's "Natural Philosophy") in slide (3). Let the direct pull at the two opposite faces of the cube be represented by P, and let P be divided into three equal parts as shown. On the other four faces of the cube let forces each equal to $\frac{1}{3}$ P act in opposite directions.

The joint action of all these forces is a direct pull equal to P tending to increase the distance between the first-named pair of faces, while no force is exerted on the other two pairs. This is equivalent to two shearing stresses at right angles to one another, each equal to $\frac{1}{3}$ P, combined with dilating stress equal to $\frac{1}{3}$ P in all directions.

The extension due to the two shearing stresses is therefore

$$P\left(\frac{2}{3n} + \frac{1}{9}k\right),$$

and hence

$$\text{Young's Modulus (E)} = \frac{9nk}{3k+n}.$$

Although no force is exerted on the lateral face this does not imply that there is no change of lateral dimensions, for the contraction due to the shearing stress is $\frac{P}{6n}$ in both lateral directions, while the dilatation in the same direction is only $\frac{P}{9k}$.

If the extension due to a direct pull is compared with the consequent change of diameter, it will be found that

$$\frac{\text{Lateral contraction}}{\text{Longitudinal extension}} = \frac{1}{2}\,\frac{3k-2n}{3k+n}.$$

This ratio, generally denoted as μ, plays an important part in many physical and mechanical problems. It is known, rather ironically, as "Poisson's Ratio"—Poisson having proved, as he thought, that the ratio was the same for all solids, and equal to one-quarter.

As a matter of fact it may have any value between $\frac{1}{2}$ and 0, according to the relative magnitudes of the volume elasticity and the rigidity.

If a solid is easily distorted but offers great resistance to compression, the sides move out or in by almost half the distance by which the ends are moved in or out, and if the rigidity is great compared to the volume compressibility, the reduction or increase of length makes hardly any difference in the diameter.

This may be shown by a simple experiment. I have here a cylinder of indiarubber and a cylinder of cork, placed between two washers on steel bolts. Indiarubber is a substance which can be easily distorted, but offers great resistance to volume compression. Cork, on the other hand, resists distortion much more than it does volume compression. It will be seen, when both are reduced to about half their original length by turning the nut on the bolt, that the diameter of the indiarubber has increased by nearly one-quarter, while that of the cork has hardly changed.

The whole of the mathematical theory of Elasticity turns on the four quantities k, n, E and μ, together with the assumption that these are constants for the range of strains and stresses contemplated in the problems.

As before mentioned, there has been very little direct experimental work on volume compressibility, but if Young's Modulus

and the rigidity coefficient are known (and both are easily determined), the volume compressibility can be deduced from the relation (derived from the expression for E in terms of k and n)—

$$k = \tfrac{1}{3} \frac{\mathrm{E}\, n}{3\, n - \mathrm{E}}.$$

All, or nearly all, the values of the coefficient of volume compression given in books of reference or in table of constants are reached in this way.

But k may also be found if the values of Poisson's and the rigidity are known, for from the expression for μ it will be seen that

$$k = \frac{2\, n}{3} \frac{(1 + \mu)}{(1 - 2\, \mu)}.$$

Poisson's Ratio is not perhaps as easily determined by direct experiment as Young's Modulus or the rigidity coefficient. In 1877, however, I employed a method which was applicable to most materials and was susceptible of considerable accuracy. This I will shortly describe and illustrate the procedure by a model.

When a flat bar is bent the material on the convex side of the neutral plane is stretched in the direction of the length and compressed on the concave side, with the result that diameter of the bar is reduced on convex face and increased on the concave. In consequence the cross-section of the bent bar is no longer rectangular, but has its faces curved in the opposite sense to the curvature of the longitudinal section. It is easy to see that the ratio of the two principal radii of curvature depends on the ratio of lateral contraction to longitudinal extension, and that if this ratio of the curvatures can be measured the value of μ for the material of the bar can be deduced.

In my own experiments the bar was bent by equal couples applied at the end; thus the bar originally straight became an arc of a circle. On the middle part of the surface a circle was described whose diameter was a little less than that of the bar itself, and at the ends of the diameters of this circle, parallel and perpendicular to the length of a bar, four fine steel wires were planted forming normals to the unstrained surface. The wires remain normal to the surface from which they spring when the bar is strained. Hence the free ends of the pair on the longitudinal diameter move apart when the bending couple is applied, while those on the transverse diameter approach one another. (See Fig. 2.)

The distances through which they moved were measured with a microscope and micrometer, and a comparison of the displacements of each pair gave the data for the calculation of μ.

The elastic properties of matter vary with the temperature, and during the last few years I have examined the changes which occur

in various metals and a few non-metallic substances from this cause. The range of temperature covered by the experiments lay between 100° C. and the temperature of liquid air. I had hoped to go as low as liquid hydrogen temperatures, but the difficulty of getting a sufficient supply of the liquid has been in the way.

The first series of experiments were on the temperature change of Young's Modulus, and the slide (6) will show the form of apparatus adopted for the purpose.

It was necessary to be able to work on small test-pieces, both on

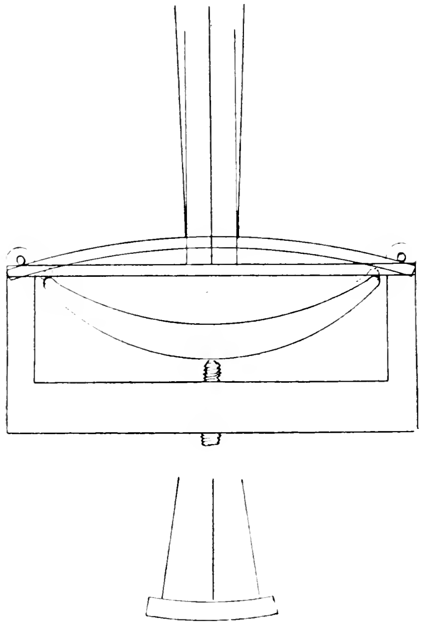

Fig. 2.

account of cost of such metals as rhodium, platinum, etc., and of the convenience of having a moderate-sized lathe for the heating or cooling liquids.

Four temperatures were generally tried for each test-piece—viz. 100° C., ordinary temperature about 14° C., a freezing mixture of alcohol and carbonic acid (–100° C.), and liquid air (–173° C.).

The test-pieces were in the form of thin plates an inch and a half long and about a quarter of an inch wide, with a thickness of one or two hundredths of an inch. The lower end of the test-piece was

clamped to a fixed support carried on a stout glass rod. The upper end was clamped to a stiff hardwood rod, wood and glass being chosen as being bad conductors of heat.

Mounted in this way the rod could oscillate (like one end of a tuning-fork), and being itself so stiff compared to the test-piece as to remain practically unbent, the force of restitution was determined by the elasticity and dimensions of the test-pieces only. The elastic quality called into play when a bar or plate is bent is Young's Modulus, and hence to determine the variations of Young's Modulus at different temperatures it was only necessary to compare the periods of oscillation of the test-piece at those temperatures, the temperature being the only condition which was altered (if the small correction due to dilatation or contraction is neglected). The actual dimensions of the test-piece and the periods do not matter, all that is required being the comparison of its periods at different temperatures. In order to make this comparison the actual periods had to be measured with considerable accuracy.

The periods employed were generally between half a second and two seconds, and if the rod is displaced and then left to itself the oscillations so started would from various causes die out before a sufficient number could be counted to give an accurate result—accurate, that is, to one part in two or three thousand. The oscillations therefore were maintained by an arrangement I had before used for an electric clock. This gave an impulse to the rod at the moment of its passing through the zero position. An impulse given to an oscillation at that time has no effect on the period, but merely increases its amplitude if there is no resistance, or if there is resistance keeps the amplitude constant by adding at each impulse the loss sustained during the previous beat.

It is shown in the slide (7). (Description.)

I have the maintainer here, but as its action could not be well shown when applied to experiments described, I have arranged it to maintain the vibrations of a pendulum the image of which will be thrown on the screen.

The current which worked the magnets of the maintainer was also used to record each oscillation on a chronograph on which a clock recorded seconds of time. In this way time could be measured to about $\frac{1}{50}$th of a second, and since from 100 to 300 or more oscillations were generally made by the rod in each experiment, the period could be well determined.

It was observed that the more fusible was the metal subjected to trial the greater was the variation of the elasticity with change of temperature, the change being always an increase of the value of Young's Modulus with a decrease of temperature.

The metals tested were (in the order of their melting point), rhodium, platinum, iron, palladium, nickel, copper, gold, silver, magnesium, aluminium, zinc, lead, cadmium, bismuth, and tin.

If the variations of Young's Modulus depended solely on the melting point, the assumption might be stated in the form—

Young's Modulus at temperature θ_1 absolute is to that at θ_2 as θ_1 is to θ_2, so that if $\theta_1 = 0°$ C. and $\theta_2 = -273$

$$\frac{\text{E } 0° \text{ C.}}{\text{E } - 273° \text{ C.}} = \frac{\text{Melting point Centigrade}}{\text{Melting point Centigrade} + 273° \text{ C.}}$$

In the full curve represented in the slide (8) the ordinates give the variations of Young's Modulus for a metal whose melting point (absolute) is indicated by the abscissa, calculated from the equation. (Fig. 3.)

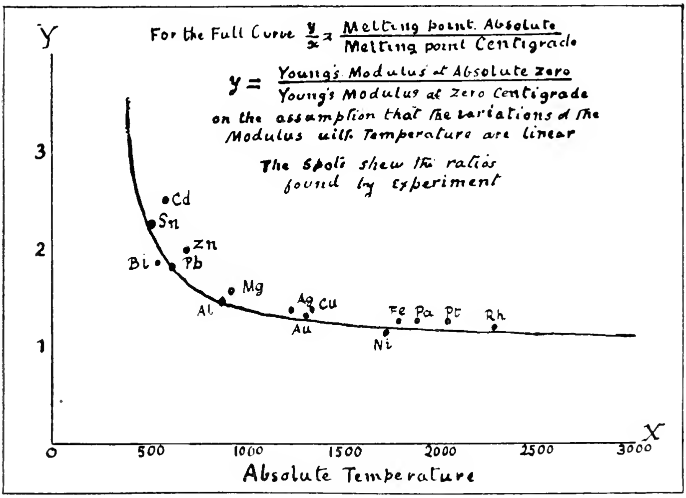

Fig. 3.

The circles show the variations actually found for the various metals above-mentioned, and though it is evident that something beside the melting point influences these results, the experimental points do show a tendency to follow the curve, though the variations found by trial are generally in excess of those estimated.

Young's Modulus being a complex quantity, depending both on rigidity and compressibility, I thought it would be of interest to try experiments in which rigidity only was involved, and this was done by timing the torsional vibrations of test-pieces in the form of wires or very narrow strips of plate.

The apparatus used is shown in the slide (9). A vertical rod suspended by a long fine wire carries a cross-arm weighted at both ends, to increase its moment of inertia; the test-piece is clamped to

the lower end of the vertical rod and clamped again to a fixed support. To avoid conduction the clamps are mounted on glass rods. The period of the torsional vibrations depends chiefly on the stiffness of the test-piece, but in part also on that of the suspension wire, and this has to be allowed for in calculating the true period due to the reactions of the test-piece itself. (Fig. 4.)

The periods of the test-piece and suspension wire combined ranged from five to ten seconds, and the oscillations when once started lasted long enough without maintenance to allow of their being accurately timed. As a matter of convenience, however, in recording each oscillation on to the chronograph, maintenance was applied in the form of an impulsive couple acting for an instant while the oscillating system was passing through the zero position. This was effected by the falling of two small weights, which were released at the proper time by an electro-magnet actuated by a current which flowed only when a fine wire point carried by the oscillating parts touched a globule of mercury, the same current being used to work the pen on the chronograph.

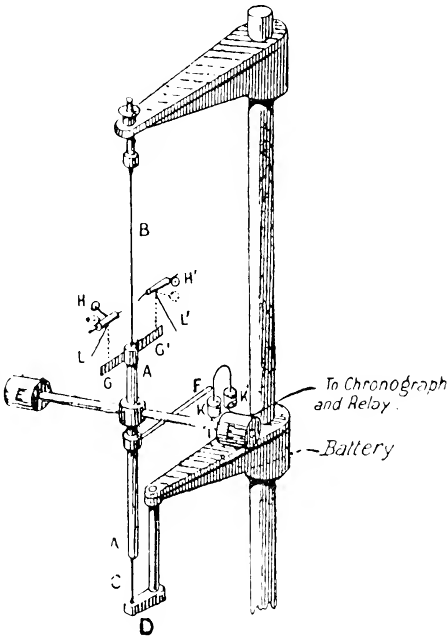

FIG. 4.

The variations of the rigidity of most of the metals used in the previous series, with the addition of tungsten, were examined in this way, and the results, as will be shown presently, did not differ much from those found for the variations of Young's Modulus.

What was noticeable, however, was the great difference which the change of temperature produced in the amplitude of the maintained oscillation and in the rate at which the unmaintained oscillation died out. In both cases the difference is caused by the variation of the viscosity, or internal friction of the material of the test-piece.

To get some measure of this internal friction I arranged a further series of experiments which, though still proceeding, has yielded some results.

Two methods of arriving at the desired information have been tried.

The first is to record not only the periods, but also the amplitudes of the free oscillations. In this method the oscillation is not maintained, and the rate at which the motion dies out gives a measure of the work lost at each instant.

The second plan is to maintain a forced oscillation of constant amplitude by the application of a known harmonic couple, and to record the amplitude of the forced oscillation and the relation between phases of this and of the maintaining couple. When these elements are known it is a simple matter to calculate the work expended in maintaining the constant amplitude.

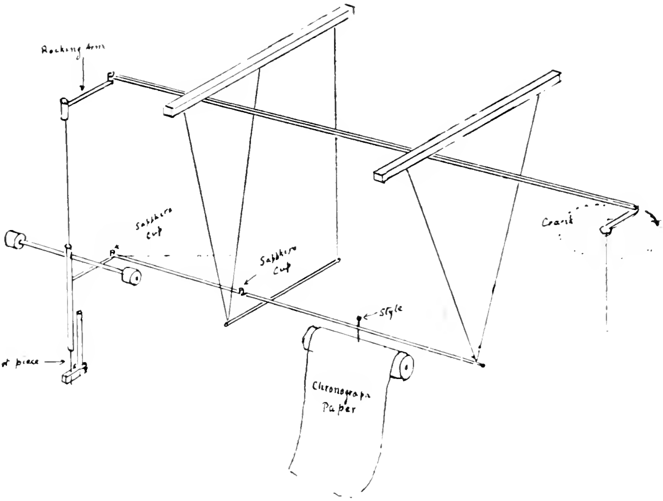

Fig. 5.

In experiments such as these, where the object is to record resistance, great care is required to exclude all sources of resistance other than that which is to be measured, and in particular to eliminate all chance of solid friction. Viscous resistance in the recording mechanism when small can be determined by separate experiments and its effects allowed for.

The apparatus employed is on the lecture table, and as far as the oscillating parts are concerned is the same as that used in the previous series, except that provision is made for the application of a forcing couple to the suspension wire, and a light horizontal arm is added to the suspended rod, from the end of which arm a connecting rod transmits the motion to the style recording the amplitudes on the

chronograph. It is in these parts that constant friction is most likely to occur.

The slide (10) shows the general arrangement. The style is mounted on a horizontal reed so hung by silk fibres as to have only one degree of freedom—namely, that of motion parallel to its length, and at right angles to the travels of the chronograph paper. (Fig. 5.) This kind of suspension I have used on a larger scale for ballistic pendulums.

I chose a reed (*Arundo phragmites*) as a style-carrier on account of its lightness and rigidity. Such reeds are practically wooden tubes with walls less than a hundredth of an inch thick, coated with a stiffening layer of silica.

One end of the reed is terminated by a fine vertical steel point, as also is the end of the horizontal arm on the oscillating rod. The connecting rod which joins the two is also a reed, at either end of which are mounted sapphire cups such as are used for magnetic compasses.

It was essential that the style should not touch the paper on which it recorded its position, and this condition I at first intended to satisfy by using a syphon-recorder of Sir W. Thomson's type. I found, however, that it was much simpler to use an ordinary induction coil, the sparks from which, if properly adjusted, left an excellent trace on the moving paper. The style, therefore, took the form of a steel needle passed through the reed with its point about a hundredth of an inch above the surface of the chronograph paper.

One of the records obtained in this way is now shown as a lantern slide. (See Fig. 6.)

This exhibits the rapid natural extinction of the oscillation in the metal tin.

In order to apply a known harmonic forcing couple to the oscillating parts a horizontal arm was fixed to the upper end of the suspension wire, and this, by a long connecting rod working from a crank of adjustable throw, was made to twist the wire harmonically through any desired angle.

The crank was driven by a small electro-motor through a reduction gear, and its speed regulated by the position given to a belt on two cones, one cone being driven by the motor, and the other, by a second belt, driving the crank shaft.

The motor itself was controlled by a centrifugal governor, which regulated the current supplied to it from the main. Each revolution of the crank shaft was recorded on the chronograph, so that the relative phases of the applied couple and the forced oscillation could be compared.

The experiments depending on oscillations have not yet been completed, but the next three slides show some of the results obtained by the experiments on the extinction of the natural oscillations.

In slide (12) these are put in the same form as in (8), and they

indicate that rigidity-variation depends on the temperature of the melting points very nearly in the same way as does Young's Modulus. (Fig. 7.)

In the next slides (13 and 14) are shown the percentage variation of rigidity of the various metals between the limits of the temperatures employed in 100° C. to −176°. It will be seen that the increase in rigidity as the temperature falls is not linear, and that there is not sufficient evidence to make even a useful guess at what the rigidity would be at absolute zero.

In slides (15 and 16) are shown the variations of viscosity for same selection of metals.

To explain these diagrams it may be stated that the oscillations

Fig. 6.

of viscous substances, where the strain does not exceed the limits of elasticity, naturally decrease at compound interest—that is, the amplitude of each successive oscillation is some constant fraction of the one which preceded it. In symbols, if A_0 is the amplitude at time t_0 and A_t the amplitude at time t, then

$$A_t = A_0 e^{-\frac{t-t_0}{c}},$$

where c is a constant, and this constant is the time in which the amplitude is reduced in the ratio of e to unity.

The ordinates of the curve shown in the slide are the proportional times in which the viscosity of the metals causes a reduction of $e : 1$

at the temperature indicated by the abscissa, the time at 100° C. being taken as unity.

Although on the whole the metals become less viscous at low temperatures, there seems to be an actual increase, or slower rate of decrease, of viscosity somewhere between 0° and − 100° C., which is particularly noticeable in the case of copper, gold and silver.

This is a matter which requires further investigation. The same metal often gave different results in the annealed and hard conditions.

There is no very apparent connection between the variations of viscosity and the melting points, although the easily fusible metals show the larger changes in this respect.

A general conclusion which I think may be drawn from these

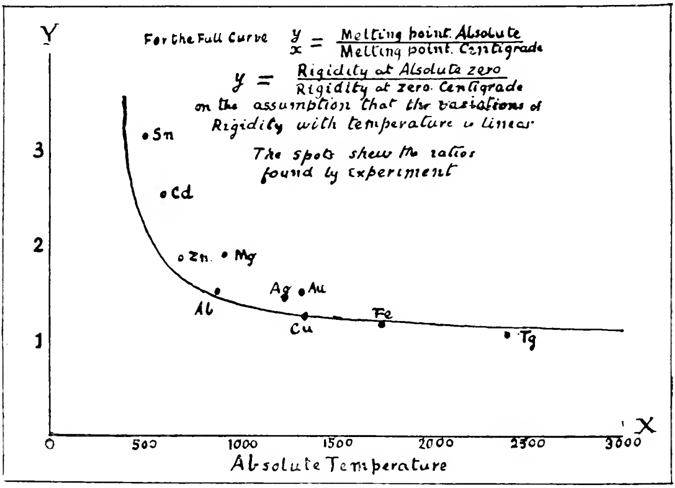

Fig. 7.

and the former experiments is that the rigidity of any metal and its variation with temperature is not much affected by the present state of the test-piece or its previous history, being nearly the same for hard, soft, cast, forged or annealed specimens; but that the viscosity, both as regards its absolute value as well as its temperature variations, is influenced by all these conditions, though not so much as are the elastic limits.

I have here an experiment which will show how small is the difference between the elasticities of hard and soft steel, and between these and wrought iron.

A bar of hard steel is laid horizontally on supports at either end. A weight can be hung from the centre and will cause a certain deflection, indicated on an enlarged scale by the pointer. On sub-

stituting a bar of the same dimensions, but well annealed, it will be seen that the same deflection is shown. Using a similar bar of wrought iron, the deflection is only a little greater.

The limits of elasticity are of course widely different in all three cases.

In engineering and structural work it is the limits of elasticity which are important to designers rather than the elasticity itself, for it is on these limits that the strength of materials depends. The elasticity enters into calculations of the reaction of springs, in which is included the deflection of loaded structures, such as girders and bridges; for all structures, whether meant to be rigid or not, are in effect springs with longer or shorter periods of vibration. It is the strain limits, however, of the materials, and not their elasticities, which decide what loads can be safely borne.

When a material is strained beyond its elastic limits it either takes a permanent set or is ruptured, and the manner in which set or rupture occurs gives rise to such qualifications as hard, soft, brittle, tough, malleable, ductile, friable, plastic, etc.

The behaviour indicated by these adjectives can be explained by the relative differences between the volume elasticity and rigidity in conjunction with the limits of strain which either kind of elasticity will withstand.

When a substance is said to be hard or soft (in the metallurgist's sense) it is implied that the rigidity is great or small. For the mineralogist hardness has another meaning, and refers to the properties of a surface as opposed to that of a volume. If one material will scratch another, the one which is scratched is considered the softer of the two, and by this test a rough and arbitrary scale of mineralogical hardness has been established, ranging from diamond at one end to talc at the other.

I believe that this sort of surface hardness is analogous to surface tension, for it is hardly to be thought that the causes which produce surface tension in fluids cease to operate in the case of solids. On this view hardness of this sort might be defined as the limit of tangential strain at a surface which can be borne without rupture. This may be very different from the strains which can be borne in the interior. For example, diamond is the hardest substance known in the mineralogical sense, but it can be easily crushed by hard steel, showing that one, at any rate, of the strain limits for diamond is less than it is for steel.

A body is brittle if, whether hard or soft, the limits both for volume extension and rigidity are small, and the characters of the fractured surface will often help to determine which of these limits is the greater; but it would take too long to enter into the details of the subject at this time.

Malleability implies that the material can be worked without rupture under the hammer; ductibility, that it can be drawn out

into wire or tube. The stresses produced by hammering are shear and volume compression, while in drawing they are shear and volume extension.

Hence it may be concluded that in a material which is malleable but not ductile, the limit of strain for volume expansion is less than the rigidity limit, and also, that body which is ductile is also malleable, but that the converse is not true.

Plastic bodies are those which when distorted do not rupture but retain their distorted shapes, and this implies that the elastic limits for rigidity are small, but that the limit for permanent set are large.

I will conclude by showing a few experiments designed to illustrate fracture by volume extension and fracture by shear.

I have here some brass rings over which damp paper has been stretched. The contraction of this paper as it is dried by the heat of the lantern will be sufficient to cause rupture.

In the stress due to contraction volume extension is predominant in the proportion of two to one.

It is impossible to foretell in what position or direction the break will occur. In an isotropic material this would be a matter of pure chance, but in the present case some irregularity in cementing the paper to the ring or in the structure of the paper itself will decide.

In the next experiment a sheet of brittle material will be broken by shear. The material used is a square of dried sheet gelatine, two of whose opposite edges are gripped by clamps, which are then made to slide parallel to one another at a constant distance.

If the sheet were thick enough to withstand edgeway pressure without folding, the applied stress would be a simple shear, and in the actual case shear is predominant. The direction of the fracture, as will be seen, is nearly at right angles to the extended diagonal.

Lastly, I will break some of the same gelatine which has had its limits of shear increased by the absorption of water, and draw your attention to the shape of the termination of the cracks which have been started.

At the end of a spreading crack the material is undergoing intense distortion, and when the limit for shear strain is much in excess of the volume strain limit, the termination of the crack is, as in the present instance, rounded.

In a note appended to this lecture the effect of the relative magnitude of the elastic constants and their limits on the character of permanent set and fracture is considered in somewhat greater detail. The subject is one on which much useful work might be done.

In conclusion, I wish to give my best thanks to the Director of the Davy Faraday Laboratory, where the greater part of the experiments on Rigidity have been carried out. The experiments, in fact, could not have been made without the facilities for dealing with low temperatures which the Davy Faraday Laboratory has afforded.

[A. M.]

WEEKLY EVENING MEETING,

Friday, June 3, 1921.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Leonard Huxley, LL.D., Editor of the "Cornhill Magazine."

Chronicles of Cornhill.*

Dr. Johnson uttered many true sayings about human nature. Among the rest he was quite right when he laid it down as a rule that even anonymous writers want to be paid well for writing well. The implication is that good writing is hard work, and good writers do not care to work for nothing, nor to do bad work. Nor are they alone in these sentiments. There exist publishers also who, while sharing the same laudable desire to be paid for their work, yet are too proud to associate their name with anything that cannot fairly rank as literature. Such a publisher was George Smith, for over half a century head of the house of Smith, Elder, to whom indeed the toils of publishing were, so to say, something of a relaxation among the vaster cares and responsibilities of his great East India business. Literature and art had early cast a charm upon him, although he was himself neither artist nor writer.

His personal intercourse with writing folk began early. While he was still in his teens, and before he had started his precocious career as the boy-publisher, his father had begun publishing the works of the budding genius, John Ruskin. This was the beginning of a close friendship and constant visits to the Ruskins' house, where George Smith was thrilled by the eloquence and imagination of young John, and touched by the devotion—sometimes pushed to a humorous extreme—of his admiring parents. Here, too, he came to know Richmond and Millais, destined long afterwards to draw for the "Cornhill"; Burne-Jones and Alexander Munro the sculptor, and among these he also found life-long friends.

A number of other literary friendships he owed to Thomas Powell. Among them were Leigh Hunt and G. A. Sala, Bohemian of the Bohemians, who used to keep his oak sported for fear of duns, but had a secret code with his friends, according to which they announced their pacific arrival by dropping a penny noisily through

* The Discourse in full is printed in the "Cornhill Magazine," Vol. LII., p. 364 (March), 1922.

the slit of the letter-box in the door; Robert Browning also, whose life-long friend he was to become.

The publication of "Jane Eyre" led to the acquaintance with Thackeray, for Charlotte Brontë, then visiting the Smiths in London, was eager to meet the man who was her literary hero, and George Smith effected the meeting by boldly asking him to dinner.

Thackeray as a writer had long been one of George Smith's admirations. At a Coffee House Sale, which he attended as a lad in his father's office, the "Paris Sketchbook" was passed round for inspection. The boy was so deeply interested in it that he utterly forgot the business upon which he had been sent. A friend got him out of his scrape. And when the opportunity of publishing for Thackeray came, the business relation, as in so many cases, became but one side of a very deep and real friendship.

George Smith was struck by an idea which promised to unite successfully two strands of popular interest; one, the novel by a great novelist, issued in monthly parts, as Dickens, for example, had been publishing his works for years past, the other, the magazine with contributions written by first-class writers, and illustrated with a couple of woodcuts from drawings by first-rate artists. Combine the two; and publish at a shilling—the price of the monthly instalment of the novel alone—the new magazine of such quality and such promise should attract a double contingent of readers. The watchword of the magazine was to give of the best, and therefore to spare no cost in getting the best. Contributions were paid for on a scale till then unprecedented: the highest payments being 12 guineas a page to Thackeray, and £583 a part to George Eliot for "Romola."

The great novelist whose name was to float the enterprise at the start was at hand in Thackeray, now a long-established friend as well as a business client. The next question was to find an editor who should combine literary reputation with organising capacity. But to find one was not an easy task. Tom Hughes, the first approached, was otherwise engaged. In the end George Smith asked Thackeray to be editor as well as to contribute the serial. Any writer would feel it an honour to write under his ægis, and if, as his publisher knew, Thackeray was not a good man of affairs, let the publisher himself stand by his side and manage all the prosaic transactions, the staff-work and commissariat, so to say, of the enterprise.

Thackeray accepted the position with enthusiasm. One more safeguard, however, was laid down by the far-seeing proprietor. Knowing well his friend's character, his occasional whimsicality, his warmth of heart which made it difficult for him to say no when his sympathies were played upon, he reserved a right of veto over all acceptances. Such a relation would have been impossible between most men; but Thackeray's nature was so generous, and George Smith's regard for him so sincere, that no misunderstanding ever arose between them.

The next point to settle was the name of the magazine. The world was familiar with magazines named after their publishers. There was "Blackwood's," the original "Maga"; there were "Colborn's Magazine" and "Chambers' Journal," and "Fraser's," to name no more, while "Macmillan's," generated by the same wave of opportunity that had suggested the "Cornhill," was in fact launched a couple of months earlier than the "Cornhill." But the name of Smith, Elder did not lend itself to such a title. Names of cities had lent dignity to the "Edinburgh Review," and the "Westminster"; Thackeray pushed the notion a step further. The storehouse and office of the magazine lay in Cornhill; let it be named after its local habitation, a name "with a sound of jollity and abundance in it." The choice was sneered at as undignified; nevertheless one magazine after another has been proud to proclaim its association with some historic point of London, from Temple Bar and Belgravia to St. Paul's and the Strand.

The cover was designed at Sir Henry Cole's suggestion by Godfrey Sykes, then a promising art student in the newly established schools at South Kensington, other specimens of whose work are to be found in the interior decorations of South Kensington Museum. As a work of art it won universal admiration in 1860, and if to-day we are critical of its subsidiary parts and of its adaptation to practical purposes, we are still conscious of the great beauty of its chief features, as well as its historical and traditional charm.

The first number is dated January 1, 1860, but was actually issued before the preceding Christmas Day. The unprecedented sum of £5000 had been spent on advertising it: its aims were set forth in the form of a letter from Thackeray to G. H. Lewes, a miniature essay on what such a magazine should be. Expectation was on tip-toe; the debut of the "Cornhill" doubled George Smith's estimate. Printed and reprinted, the first number reached the unprecedented circulation of 120,000. George Smith, with his customary openhandedness, promptly doubled Thackeray's salary as editor. Thackeray could not contain his enthusiasm: his spirits boiled over, and to escape the excitement of London he dashed over to Paris to stay a few days with his American friend, J. T. Fields, who has left an amusing description of the visit.

It may be frankly avowed that this first flood of popular success was not maintained, though the magazine redeemed the promises with which it set out, and which were neatly summed up in a verse of Father Prout's Inaugural Ode:—

"With Fudge, or Blarney, or the Thames on fire
  Treat not thy buyer;
But proffer good material—
  A genuine Cereal,
Value for twelve pence, and not dear at twenty,
Such wit replenishes thy Horn of Plenty."

Curiosity for the new thing had impelled many to buy who found that the bulk of the material was, to borrow a modern Americanism, too highbrow for their real taste. Moreover, the early death of Thackeray himself on Christmas Eve, 1863, following his retirement from the editor's chair in May 1862, deprived the magazine of one of its leading attractions. Then arose, what Mrs. Browning had early foreseen, the competition of other magazines, which expanded year after year, until with the vast growth of an uncritical reading public, a fresh order of magazines, more ephemeral in character, in their turn devoured the earlier competitors of the "Cornhill." That the "Cornhill" itself survived the years of stress is due to the devotion of George Smith's successors to his creation, and the ideals it represented. According to these ideals, it was to be a literary magazine not in the sense of merely discussing literary subjects, but in the sense of treating each subject with the responsiveness of thought to feeling and of word to thought, which differentiates literature from that which is not literature. Being neither a journal nor a review, the "Cornhill" stood aside from current politics, book reviewing, ephemeral topics, the clash of controversial opinion as such, along with theology. In all else it looked for form as well as substance, for warmth as well as light. Essays might teach, but they should not be didactic; descriptions must not be a catalogue of experiences, but must be projected anew through living facets of a personality.

The essay, then, has always been one of the cardinal features of the "Cornhill." The other was the serial. Both began with Thackeray, and both have continued in unbroken descent to the present time. Save for an occasional interval of a month or two, between the end of one and the beginning of another, it has been steadily maintained; indeed, for many years two serials were running simultaneously, and sometimes they were overlapped by a third.

It is remarkable to see what an array of first-class novelists contributed to pages of the "Cornhill." Some had already achieved fame, like Thackeray himself, and Trollope, and George Eliot, and Wilkie Collins; others, like Lady Ritchie from her very first effort, and like Stevenson and Thomas Hardy, Stanley Weyman, Conan Doyle and Merriman, found the "Cornhill" their chief stepping-stone to popular appreciation. Here are Mrs. Gaskell, George Macdonald and George Meredith, Charles Lever and Charles Reade, William Black, and R. D. Blackmore, Mrs. Oliphant and Henry James; here is the sombre genius of George Gissing, the fun of F. Anstey, and George A. Birmingham, whose pseudonym conveys a jest in its very initials; here are Anthony Hope, A. E. W. Mason, and H. A. Vachell, Mrs. Humphry Ward, and Lady Clifford (Mrs. de la Pasture) and the author of "Elizabeth and her German Garden," to name no more; in short five out of six have been novelists of the front rank.

Among the general articles one or two features of the "Cornhill" may be noted which have marked it from its earliest days. Anne

Thackeray's pathetic sketch, "Little Scholars," is the forerunner of many articles which treat the problems of social betterment, down to the latest schemes for making life happier, as tried in other countries and possibly to apply in England.

Science, too, is constantly set forth in a form easily understanded of the people, and linking its abstractions with other branches of life and thought, first by G. H. Lewes and the astronomer Richard Proctor, then by Grant Allen, and notably by W. A. Shenstone, the only public school master of the day who was also F.R.S.

Note also the miniature biographies. They belong especially to the latest period of the magazine: personal sketches of a writer by a fellow-writer and a sympathetic friend.

The rule at present is for articles to be signed; the exception that they should be anonymous. In the earlier days of the "Cornhill" it was the reverse. Signatures appear only in the case of eminent poets, deceased writers, such as the Brontës, and later, one or two novelists and specialists. The change took place after the editorship of James Payn, who held that so revolutionary a measure ought not to be undertaken lightly or inadvisedly. We understand the rule of anonymity better in the case of a newspaper or a Review which impersonates a point of view. It is strange to us that the story, the sketch from life abroad or at home, the literary study, all so personal in their point of view, should be thus impersonal. Even Thackeray signs but one of his contributions; Matthew Arnold is anonymous on two occasions, and Anthony Trollope's name does not appear at all with his first novel, "Framley Parsonage," and indeed only on the last instalment of "The Small House at Allington."

The "Cornhill" has passed through three stages. The first lasted till the end of Leslie Stephen's editorship in 1883, following the general lines already described. Under James Payn the magazine took a more popular form. Fiction, whether as serial or short story, was to predominate, and the price was reduced from a shilling to sixpence. The change worked well for a considerable time, though it was impossible to keep up the illustrations, which, after three years, were abandoned, for once more a new competition came into play. The process block swept away the woodcut, and produced the latest type of cheap illustrated magazines. And thus, although Payn was very successful in discovering new writers of excellent fiction, his experiment gradually found itself floating on an ebb tide, and on his retirement in 1896 the "Cornhill" entered on its third stage by returning to the price of a shilling, increasing the number of its pages, and resuming the older and larger proportion of literary and general articles, even while as a rule keeping two serials running together.

During the war there was a long struggle with the increased cost of production. This was met by reducing the number of pages of the magazine. At the same time its circulation increased greatly,

and a large order was placed for distribution among the armies on active service, where it was much sought after. In peace, it seems, the reign of popular sensationalism spreads widest; in war, it is challenged by the yet cruder sensationalism of reality, and a larger proportion of the public turn away to a finer art and a clearer atmosphere.

To turn to another chapter of "Cornhill" history: the relation of the editor or proprietor of the magazine to his contributors, and the personality of the successive editors. Up to the early days of the "Cornhill," writers of every kind, including many who may nowadays be gibbeted as eminently Victorian, were far more unconventional and Bohemian in their social ways than their latter-day critics.

Thackeray, who had lived much in these irresponsible circles, never lost a streak of this careless Bohemianism. When literary business had to be transacted with George Smith, it could never be so well done, he averred, as after a capital dinner at Greenwich, animated by a bottle of his favourite brown hock at 15*s*. a bottle.

These genial meetings were the germ, no doubt, of the occasional dinners to a gathering of contributors. These "Cornhill" dinners do not constitute a parallel to the weekly "Punch" dinners. The latter consisted of the "Punch" staff; they were preliminaries to the work of making up the next number; the former originally took place every month at George Smith's house, but afterwards were held at no regular interval, and their object was amenity, not business. They formed a pleasant means whereby George Smith kept up with old contributors, made acquaintance with the new, and brought old and new into touch with one another in a manner which made for the personal continuity of the "Cornhill" tradition. And as the founder of the "Cornhill" began, so his successors followed.

The only contemporary reference to these dinners is to be found in a Roundabout Paper, "On Screens in Dining Rooms," a strong but dignified rebuke to Edmund Yates, in later days the cynical owner of "The World," who was at feud with Thackeray, and made a spiteful attack upon the "Cornhill," Thackeray's magazine, and its proprietor, Thackeray's friend, using a garbled story of what took place on one of these private occasions.

Thackeray left a deeper mark upon the "Cornhill" than the shortness of his editorship might suggest. During the two and a half years that he was editor, and the year and a half that followed, his long novels and his many essays gave concrete form to a great part of the original programme laid down in conjunction with George Smith, and stood firm as an examplar to his successors. His fellow-worker remained to watch over the fortunes of the "Cornhill" for nearly forty years, and passed on his ideas unimpaired to his successor, while Thackeray's own son-in-law, Leslie Stephen, was editor for a decade or more. It is not too much to say that the first impulse at the heart of the "Cornhill" continued essentially through the years.

When Thackeray resigned, the editorship was put in commission for a time. With G. H. Lewes, first Frederick Greenwood, then Dutton Cook successively joined George Smith on an editorial committee, with a four years' interlude of sole editorship by the hard-working Greenwood, until he turned to editing George Smith's new venture, the "Pall Mall Gazette." Finally, in 1871, Leslie Stephen was appointed editor, and George Smith was content to rest from active participation in the work, leaving the entire management to Stephen.

But although George Smith in his reminiscences speaks modestly of a "commission" of managers during this period, he seems to have been the main directing power himself, "the Carnot of our Recent Great Victories," as Thackeray had called him, and it was through him that many of the important contributions were secured, from Anthony Trollope and Mrs. Gaskell to Charles Lever and George Meredith.

To this period belong two great names which stirred the "Cornhill" circle deeply, Ruskin and Matthew Arnold. Ruskin in 1860 contributed the opening chapters of "Unto this Last," with its inversion of the current political economy, saying that the end of science is not the production of wealth, but its distribution. So loud was the clamour raised against these heretical doctrines, often obscured as they were by paradox, that the "Cornhill" was constrained to stop the series after the fourth number, and Ruskin wrote no more for the "Cornhill."

Far more fruitful in intellectual results and in the prestige brought to the magazine were Matthew Arnold's contributions, which extended from 1860 to 1879. Arnold gave nearly all his important work to the public in the pages of the "Cornhill," for there, as he sagely remarked, he gained not only the best pay, but the widest audience.

Leslie Stephen was editor from 1871 to 1883, when, just as Greenwood had gone to edit the "Pall Mall Gazette," so Stephen went to edit Smith's other great undertaking, the "Dictionary of National Biography." Leslie Stephen, who was an old contributor to the magazine, helped largely by his own pen, especially in his "Hours in a Library," to make his editorship the palmiest period of the literary essay, though, fearing, perhaps, to give his audience "too much Stephen," his own essays were mostly unsigned. And while the fiction kept up its quality, Stephen gathered round him a company of other brilliant essayists to join his greater contemporary, Matthew Arnold, who remained an occasional contributor, from John Addington Symons to Robert Louis Stevenson, whose new blazon of R.L.S., following the familiar initials L.S., did not stand for "the Real Leslie Stephen"—so the editor confided to a friend—but for "a young Scot whom Colvin has discovered." And although one essay at least of R.L.S. was rejected, L.S. deserved well of his readers and of the

whole world of English letters by giving them the greater part of "Virginibus Puerisque," and of "Familiar Studies of Men and Books." In the preface to the latter, Stevenson records his debt of thanks to the "Cornhill": "I was received there in the very best society, and under the eye of the very best of editors."

Payn stands out as a picturesque character in his position as editor. It was not only that he was a skilful writer of essays and of stirring fiction, with a keen eye for a telling situation and well-woven plot, but his bright personal geniality, which set at nought suffering and ill-health, his unselfish friendship, his delight in discovering new talent and helping beginners, won him universal affection as well as respect.

As editor of the "Cornhill" for thirteen years from 1883, he picked out and brought forward such writers after his own heart as Mr. Stanley Weyman, Sir A. Conan Doyle, Henry Seton Merriman, Sir H. Rider Haggard, and F. Anstey.

To James Payn succeeded Mr. St. Loe Strachey, who threw himself with his customary vigour into the task of reorganisation which had been determined upon, bringing back the "Cornhill" to very much its original type, only without illustrations. But his hand was not long at the helm: a couple of years later he joined the "Spectator," and his place was taken by Reginald Smith.

Now began the longest period of a single editorship. It lasted almost eighteen years, and till the last days of his life his object was to maintain the ancient prestige of the literary inheritance which had passed into his keeping. Another tradition left by his father-in-law, his genial hospitality and love of his fellow-men rendered easy of fulfilment. It was all but invariably true that when through his business he gained a client, he made a life-long friend. He was adviser, friend, and helper in personal or business matters, and in business, where his own interests were concerned, endlessly scrupulous that the other man should be considered first, and every doubtful point be read in his favour. Towards a client his position was that, he felt, of both partner and trustee, and honour was his most every-day companion. His own tastes and education, his many friendships dating from Eton and King's, his previous career as a working barrister, conspired to give him a very practical knowledge of men and things and books, and his suggestions and counsel on the MSS. he read were often of solid service to his writing friends.

Reginald Smith not only drew closer the ties with Merriman and Mr. Stanley Weyman—still, I am proud to say, a steadfast contributor—which had been initiated under James Payn, but gathered into his friendship a new circle of writers. Among these let me name two only, who, alas! have recently passed away. To both Lady Ritchie and Mrs. Humphry Ward he was for twenty years or more a zealous guardian of their interests, trying, as he often put it, to be a buffer between them and business worries, so as to leave their

genius free to play in its proper sphere. To both he became, so to say, a friend by inheritance. Mrs. Ward was by comparison a recent friend of his father-in-law, who had begun his long friendship with her with the publication of "Robert Elsmere" but a few years before Reginald Smith joined the firm. The first link with Lady Ritchie dated from the period before the founding of the "Cornhill."

The fine series of essays he provided ranged from "The Etchingham Letters" and "Pages from a Private Diary" to a "Londoner's Logbook" and Lady Ritchie's "Blackstick Papers." It was his discernment which brought out the essayist quality in his old schoolfellow and fag, Mr. A. C. Benson.

Again, he "discovered" Dr. W. H. Fitchett, bringing into being those "Fights for the Flag" and "Deeds that Won the Empire" which have so long stirred the blood of our youth, and, incidentally, crystallised the nascent patriotism of young Australia.

In order to cast the "Cornhill" net the wider in search of topics and writers, Reginald Smith used to invite two or three experienced friends to the literary council of their "Oval Table," as it was called; and here, amid much good talk, as might be expected, for example, from Sir E. T. Cook and Sir Sidney Low, Canon Beeching and A. C. Benson, were hatched the plans of various series, from Science to Household Budgets, and, later, of the literary competition, under the ominous name of "At the Sign of the Plough," with which the "Cornhill" titillated its ambitious readers and sometimes distracted its examiners and editor in final adjudication.

In Reginald Smith's time also fell two "Cornhill" celebrations, the Jubilee of the magazine and the Thackeray Centenary, the one in 1910, the other in 1911. The personal touch in their commemoration linked the "Cornhill" with its earliest beginnings through the living words and gracious presence of Lady Ritchie and of Mrs. George Smith herself.

Looking back over sixty-two years of "Cornhill" history, the question may be asked, as E. T. Cook asked it in his article in our Jubilee number, whether there is any common touch in the 750 numbers of "Cornhill" which makes a unity of essence amid the diversity of matter. Among the bewildering diversities of its miscellaneous material that almost defy a synthesis, he confesses that on a general retrospect he seems to have a clear impression of a certain unity.

"The note [he says] of the 'Cornhill' is the literary note, in the widest sense of the term; its soul is the spirit of that humane culture, as Matthew Arnold describes it in the pages, reprinted from the 'Cornhill,' of 'Culture and Anarchy.' . . . The form in which this spirit has most particularly expressed itself in the pages of the 'Cornhill' is the essay—not necessarily the essay on literary subjects, but the essay which, whatever its subject, treats it in the temper of humane letters."

And this, he adds, is "the Thackeray touch," which has never forsaken the "Cornhill."

Herein, then, lies the secret of the tradition which the "Cornhill" has received from its literary ancestors, and trusts to hand down to its literary descendants. In the art of letters, as in the art of life, form holds equal place with substance. Literature is the tool of thought, the weapon of feeling, and in tool or weapon perfection of form excludes clumsiness and excesses that are equally ugly and ineffective. What we call style enriches the thought and clarifies the feeling which it unites. If, as Buffon said, style is the man himself, the impersonal presented through the personal sum of feeling, experience and reflection, then it is the cumulative choice of work endued with this quality that produces "Cornhill's" characteristic spirit of humane letters. To preserve this spirit is our trad tion and our endeavour; to let it perish would be treachery to the present as well as to the past.

[L. H.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1921.

Sir James Crichton-Browne, M.D. LL.D. J.P. F.R.S., Treasurer and Vice-President, in the Chair.

Miss Gertrude Caton-Thompson,
Sir John Collie, M.D. C.M.G. J.P.
Norman George Hallett,
Henry Rondel Le Sueur, D.Sc.
Mrs. Sidney Turner,
James Whitehead,
Leonard Williams, M.D.

were elected Members.

The Special Thanks of the Members were returned to Sir David Salomons, Bart., for his valuable presents of a privately printed Life and Study of A. L. Breguet, Member of the Academy of Sciences, Paris; Arago's Watch; A Mysterious Watch; Watch, by Leroy, of Paris, 1820; First Working Aneroid, made by Vidi, 1857; and Models illustrating the Development of the Chick; and to Sir Humphry Davy Rolleston, for his valuable Gift of a Davy Safety Lamp.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, June 10, 1921.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

Arthur Gordon Webster, D.Sc. LL.D. Hon.M.R.I.,
Professor of Physics, Clark University, Worcester, Mass., U.S.A.

Absolute Measurements of Sound.

It is now more than thirty years since it occurred to me to devise an instrument that should be capable of measuring the intensity or loudness of any sound at any point in space, should be self-contained and portable, and should give its indications in absolute measure. By this is meant that the units should be such as do not depend on time, place, or the instrument, so that, though the instrument be destroyed and the observer dead, if his writings were preserved another instrument could be constructed from the specifications and the same sound reproduced a hundred or a thousand years later. The difficulty comes from the fact that the forces and amounts of energy involved in connection even with very loud sounds are extremely small, as may be gathered from the statement that it would take approximately ten million cornets playing *fortissimo* to emit one horse-power of sound.

Before we can measure anything we must have a constant standard. In sound we must construct a standard which emits a sound of the simplest possible character, which we call a pure tone; it will be like that emitted under proper conditions by a tuning-fork, which is described by saying that the graph representing the change of pressure with the time shall be that simple curve known as the sinusoid or curve of sines. From this connection we say that the pressure is a harmonic function of the time. Unfortunately, the pressure change is so small that at no point in a room, even when a person is speaking in a loud tone, does the pressure vary from the atmospheric pressure by more than a few millionths of an atmosphere. Thus we require a manometer millions of times as sensitive as an ordinary barometer, and, in addition, since the rhythmic changes occur, not once in an hour or day, but hundreds of times per second, if we wish the gauge to follow the rapid changes accurately, we have many mechanical difficulties.

The problem of a standard of emission has been solved by a

number of persons, including Prof. Ernst Mach and Prof. Ludwig Boltzmann, and Dr. A. Zernov, of Petrograd, a pupil of the celebrated Peter Lebedeff. The problem of an absolute instrument for the reception and measurement of a pure tone has been also successfully dealt with by a number of investigators, among whom may be mentioned Prof. Max Wien, of wireless fame, the late Lord Rayleigh, and Lebedeff. But there remains a third step in the process, which is as important as the first and second. Given the invention of the proper standard source of sound, which I have named the "phone," because it is *vox et praeterea nihil*, and of a proper measuring instrument, which should evidently be called a phonometer, there still remains the question of the distribution of the sound in space between the phone and the phonometer. Any measurements made in an enclosed space will be influenced by reflections from the walls, and, even if we had a room of perfectly simple geometrical form, say cubical, and were able to make the instruments of emission and reception work automatically without the disturbing presence of an observer, it would still be impossible to specify the reflecting power of the walls without a great amount of experimentation and complicated theory. Nevertheless, this is exactly what was done by the late Prof. Wallace C. Sabine, of Harvard University, who employed the human ear as the receiving instrument. Those who have made experiments upon the sensitiveness of the human ear for a standard sound will immediately doubt the possibility of making precise measurements by the same ear at different times, and particularly of comparing measurements made by one ear with those made by another. Nevertheless, Sabine attained wonderful success, and was able to impart his method to pupils who carried on his work successfully, so that he was able to create the science of architectural acoustics and to introduce a new profession. Still, the skill that required three or four months to attain by Sabine's method may be replaced by a few minutes' work with the phonometer.

In order to avoid the influence of disturbing objects, the observer should take the phonometer to an infinite distance, which is manifestly impossible. The method employed was to get rid of all objects except a reflecting plane covered with a surface the coefficient of reflection of which could be measured. For this purpose the teeing ground of a suitable golf course was used. With the present instrument it can be determined in a few minutes, if there is no wind.

In 1890 I proposed to use a diaphragm made of paper, which should be placed, shielded on one side, at the point where the sound was to be measured. In order that the effect of the sound should not be distorted, the membrane, instead of having to do any work, as in the case of the diaphragm of the phonograph in digging up the wax, or in that of the microphone in compressing the carbon, was to be perfectly free, but was to carry a small plane mirror cemented on at its centre. In close juxtaposition and parallel with this was the

plane side of a lens which, viewed in the light from a sodium flame, was to give Newton's rings or interference fringes. Of course, when the sound falls upon the diaphragm the fringes vibrate rapidly and disappear from sight.

By the introduction of a Michelson optical interferometer, two of the difficulties of this instrument were overcome—namely, (1) that of adjusting the lens so that it would not strike the vibrating mirror, since the mirrors in the interferometer could be as far apart as one pleased: and (2), more important still, it permitted the use of fringes in white light, so that it was possible to use gas, incandescent, or arc light with excellent effect. A further improvement was introduced by the use of a thin plate of mica for the diaphragm.

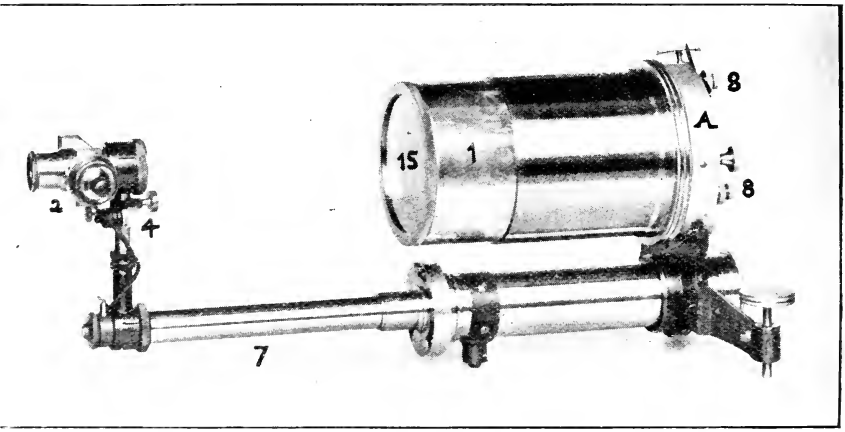

Fig. 1.—Phonometer. (Interferometer not shown.)

To obtain the sensitiveness necessary to measure sounds of ordinary intensity, the property of resonance is employed twice—i.e. a system of two degrees of freedom is used. First, the plate resounds to a sound more strongly as it is tuned more nearly to it; and second, a resonator that can also be tuned is put behind the plate. The sound entering by the hole in the resonator is magnified by the tuning, and acts upon the plate, which is also tuned. A graph can be plotted in which one co-ordinate represents the stiffness of the plate, or rather what may be called the mistuning, which is the stiffness lessened by the product of the mass by the square of the frequency. The other co-ordinate represents the corresponding quantity for the resonator, the stiffness of which depends simply on the volume into which the air is compressed, while the effective mass depends on the dimensions of the whole, and its damping on the sound radiated from the mouth. It is then found that the tuning

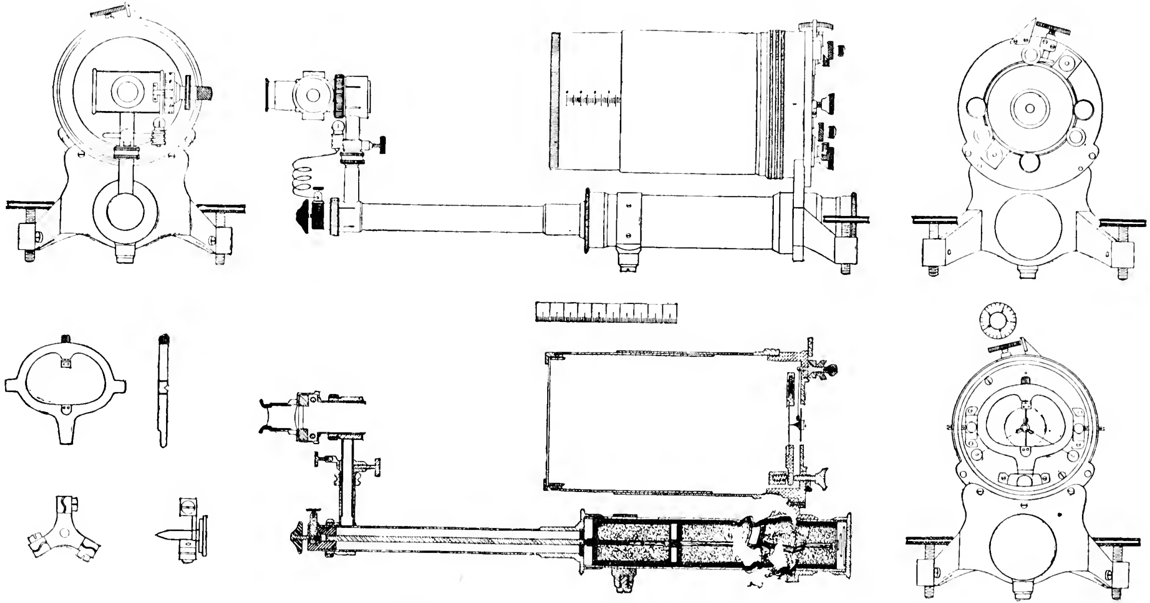

Fig. 2.—Parts of the Phonometer.

should not be such as to make the representative point occur at the middle of the figure, making both mistunings zero, but that both mistunings should be of the same sign and a certain magnitude, depending on the coefficients of damping of the two degrees of freedom of the coupled system. The mathematical theory is precisely that of a wireless receiver. The ultimate sensitiveness depends on the smallness of the damping of the plate.

The apparatus as it was built several years ago was mounted upon a heavy bronze stand, covered at the back by a heavy bronze cover to keep out the sound, while the three shafts turning the screws of the interferometer adjustment protruded through sound-tight fittings. Upon the front of the instrument a properly tuned resonator was attached, and at the side was a small incandescent lamp with a straight, horizontal filament, an image of which was projected by a lens upon the first mirror of the interferometer. Upon this was focused a telescope, giving in the reticule an image of the horizontal. straight filament, crossed by the vertical interference fringes seen with white light. In order to get these the plate must be in the proper position within a few hundred-thousandths of an inch. The objective of the tuning-fork was carried by a tuning-fork which oscillated vertically, tuned to the pitch of the pure tone to be examined, and this, combined with the horizontal motion of the fringes, resulted in a figure of coloured fringes in the form of an ellipse. On slightly mistuning the fork, the ellipse could be made to go through all its phases, and when it was reduced to an inclined straight line its inclination was read off on a tangent scale. The amplitude of the compression of the air in the sound was then directly proportional to the scale-reading.

While the interferometer is still used for calibration, the movement of the diaphragm is recorded for actual measurements by a thin steel torsion strip carrying a concave mirror. A lamp with a vertical, straight filament is viewed through a telescope into which the small mirror focuses the image of the filament on the reticule, and a magnification of from 1200 to 1500 is used, so that the sensitiveness is about the same as with the interferometer.

At first the only method of tuning was the clumsy one of changing the mass of the diaphragm by adding small pieces of wax. This was not capable of continuous variation. Now the diaphragm has been discarded and replaced by a rigid disc supported by three steel wires in tension. The disc is made of mica or aluminium, and is carried by a little steel spider containing three clamps to hold the wire. The tension is regulated by three steel pegs, one of which is controlled by a micrometer screw. The disc is placed in the circular hole through which the sound enters the resonator. This has the advantage of reducing damping very largely, and thus of increasing the sensitiveness enormously. The instrument now competes with the human ear, and can be tuned over two octaves or more.

This sensitiveness can be demonstrated by projecting the coloured interference fringes on a screen and singing faintly in a remote part of the room, when the fringes will disappear. Using the telescope end of the apparatus, the instrument will indicate the sound of a tuning-fork when one can scarcely hear it. It is obvious that the disc may be made the diaphragm of a telephone and thus increase its sensitiveness. In fact, Prof. King has used such a telephone to record wireless messages with great success. He has also invented another sort of tunable diaphragm composed of a stretched steel membrane with compressed air behind it, which enables it to be tuned continuously, but over a smaller range.

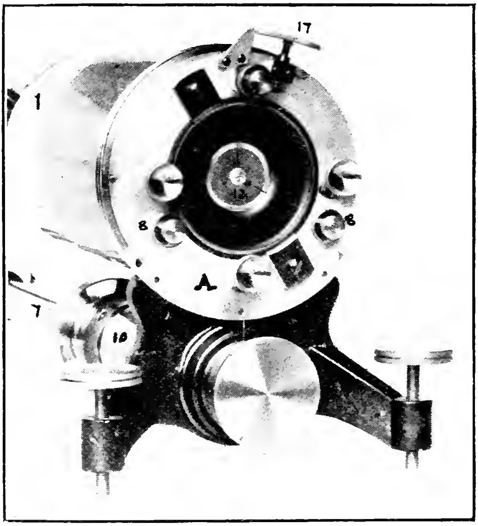

FIG. 3.—FRONT VIEW OF PHONOMETER WITH ANNULAR OPENING.

I now come to the source of sound—the phone. This has been reduced to a reversed form of the phonometer. The disc is driven by an interrupted or alternating current by means of electromagnets, and tuned like the phonometer. Its excursion is measured by a powerful microscope, and the emission of sound is known in absolute measure. It is now driven by a triode valve tube, in the manner suggested by Prof. W. H. Eccles, of Finsbury Technical College, London, for a tuning-fork. This has been worked out for me by Dr. Eckhardt at the Bureau of Standards in Washington.

The third part of the investigation involves a determination of the coefficient of reflection of the ground. The phone is set at a convenient height, and the phonometer at a convenient distance.

Either is then moved along at a constant height and the varying deflections of the phonometer are read while the sound remains the same. Interference sets in between the direct sound and its image reflected in the ground, and the existence of a minimum is obvious to the most naive observer by the ear alone. The reflection of either grass or gravel was found to be about 95 per cent., while, with a most carefully deadened room, the walls of which were covered with thick felt, there was perhaps 20 per cent. reflection. The whole measurement at both ends and the transmission checks up with an accuracy of about 2 per cent.

With this apparatus all sorts of acoustical experiments may be performed. By attaching to the phonometer a long glass tube or antenna, it has been possible to explore all sorts of places, such as the field within a horn or tube lined with an absorbent substance. The transmission of sound through fabrics, walls, and telephone booths may also be quickly examined. The instrument is used by psychologists and by telephone and acoustic engineers, and is of interest to navigators. An interesting by-product is an instrument for showing the direction of an acoustic signal in the fog. It has been called a phonotrope, on the analogy of heliotrope, which turns to the sun. It consists of two equal horns which bring the sound to the opposite sides of the disc. When the whistle blows, the band of light spreads out, and on turning the instrument it closes to zero when the sound is directly ahead. Thus at several miles the direction is given to within two or three degrees.

Finally, let us consider that mystery of sound, the violin, which has been studied by Prof. Barton, of Nottingham, and by Prof. Raman at Calcutta. This may be described by the engineer as a box of curious shape, made of a curious substance, wood, of variable thickness, with two holes of strange figure to let the sound out of the resonating box. The latter is actuated by a curious substance, catgut, made of the intestines of a sheep, and set in vibration by another curious substance, the tail of a horse. Yet from this wonderful box we get the most ravishing sounds, which affect profoundly the emotions of the most civilised. Yet the physicist reduces all musical instruments to combinations of resonators with strings, membranes, bars, plates and horns. The mathematical theory of strings was given by Euler two hundred years ago, of bars and plates less than a hundred years, of resonators by Helmholtz and Rayleigh, and I have recently added a theory of horns which, while only approximate, works well in practice, and investigations are now being carried out by such methods on vowels and the violin.

[A. G. W.]

WEEKLY EVENING MEETING,

Friday, January 28, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. M.R.I.,
Fullerian Professor of Chemistry.

Cloudland Studies

THE measurement of sky radiation has received attention for at least a century and a half. Among the earliest investigators was Leslie, who in 1818* obtained some very striking results by novel applications of his differential thermometer or pyroscope. Fig. 1 is a reproduction of the plate published by him. By the silvered reflecting cup the "aerial pulses" he wished to measure were focussed on one of the bulbs of the thermometer, while the other bulb took the temperature of the surroundings. Very considerable effects were thus recorded.

The instrument so employed Leslie termed an aethrioscope, which "extends its sensations through indefinite space, and reveals the conditions of the remotest atmosphere." He could direct it to different regions of the sky, and found the effects to remain equal from the zenith down to about 20° inclination. The "cold pulses shot downwards from an azure sky" sometimes produced as much as 5° C. difference between the two bulbs of his instrument. He speaks of "the tide of heat vibrated from a surface" which was "propagated through the aerial medium by some peculiar process," although he regarded this "pulsatory emission" as "auxiliary to the other modes of restoring the equilibrium," adding that it "contributes a very small share only towards the general effect." He also showed that with the reflecting cup of his instrument directed downwards, the clear sky above produced no effect until a polished silver tray was held between the open reflecting cup and the ground, when a negative effect of $2\frac{1}{2}$° C. was produced, reduced to 0·2° C. if a sheet of glass were used. If the tray was filled with water the effect was entirely extinguished. The altitude of the cloud layer he regarded as the important deciding factor. A range of low clouds acted as a complete screen, as seen when the instrument, exposed and equilibriated under a clear sky, responded instantly to the passage of a

* *Proc. Roy. Soc. Ed.*, viii.

dark cloud. In general "the frigorific action was found always to diminish as the clouds descend."

Pouillet, in 1837,* in conjunction with his measurement of the intensity of solar energy, devised an "actinometer" for determining

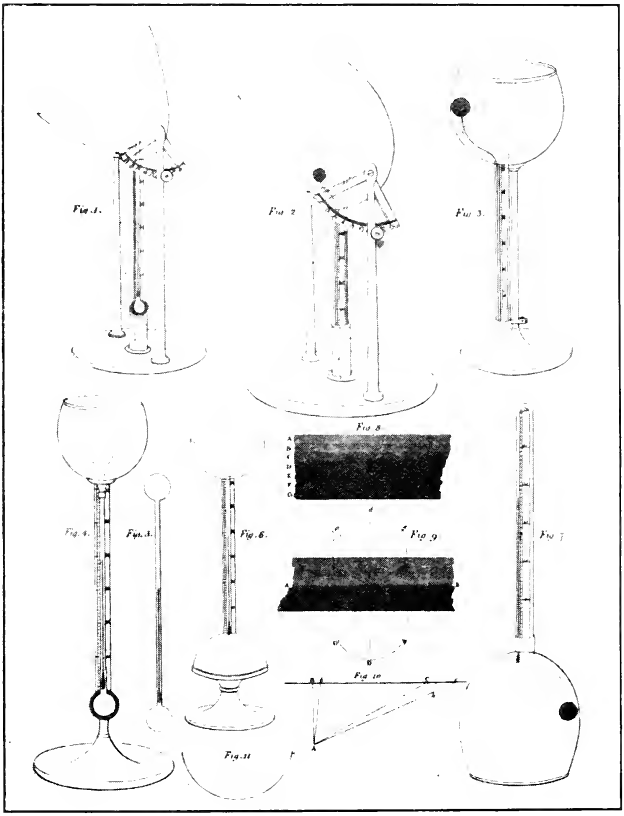

FIG. 1.

the temperature of the zenith or upper atmosphere. This instrument (Fig. 2) consisted simply of a thermometer resting on a bed of layers of swansdown interleaved with thin discs of polished silver. a

* *Comptes Rendus*, July 4, 1838, translated in Taylor's *Scientific Memoirs*, iv. 1896, p. 44.

heat-protective layer being thus formed between the thermometer bulb and the ground. There was, in addition, a polished silver rim as an aid to obtaining quiet conditions immediately round the thermometer bulb, this rim being pierced with several small holes to ensure a quiet circulation of air.

With this actinometer he endeavoured also to deduce the temperature of space, or the amount of the planetary heat falling on the earth. His results in this case are untenable, and indeed were based

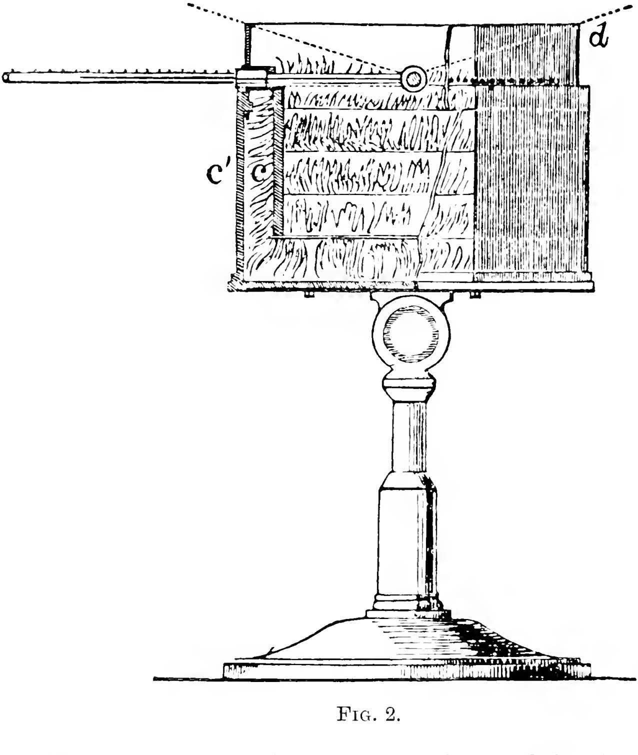

Fig. 2.

on unsafe extensions of the laws of cooling as deduced by Dulong and Petit for limited conditions, and also upon the absorptive properties of diathermanous envelopes, which were largely illusory. On the other hand, his value for the solar constant is much closer to the latest accepted value than the figures obtained by several experimenters since his time. His values for the equivalent zenith temperature were obtained by a careful calibration of his actinometer exposed to a very large superimposed cold dome at known temperature. These measurements gave him an empirical expression connecting

the observed lowering of temperature of the actinometer thermometer with the known temperature of the cold dome. This empirical expression was then applied to the readings obtained by exposure to the night sky, and the corresponding zenith temperature obtained. His empirical result was that the observed lowering of temperature of the actinometer thermometer below that of the adjacent earth was 4/9 of the actual amount of the "zenithal enclosure" below the earth temperature—i.e. :

$$\left[Z = t - \frac{9}{4} d \right]$$

where Z = temperature of zenith (or cooled dome) ; t = temperature of earth ; and d = observed lowering of temperature of the actinometer. Some of Pouillet's results are given in Table I.

Melloni, well known in connection with measurements of heat transmissivity, compared the readings of two thermometers exposed to the night sky, one of the thermometer bulbs being blackened. He found that the blackened thermometer under clear skies was cooled relatively to the plain one by an amount which was almost independent of the temperature of the air. This surprising result has been discussed and receives support in the course of an extended study of the problem of atmospheric and nocturnal radiation carried out by Anders Ångström and many others associated with the Smithsonian Institution in measurements of the distribution of solar energy.* These modern methods are based on the use of various forms of electrical thermometers or pyrometers, in which thin blackened strips of metal, forming part of a measuring circuit, are cooled or heated by exposure to the sky or sun, as the case may be, and the alteration in temperature compensated by electric energy whose value is very accurately determined. The absolute value of the radiation exchange of a strip of known dimensions is then deduced by applying the Stefan "fourth power" radiation law.

Measurements of the solar radiation were made in this way at Washington in 1902, and were repeated later at Mount Wilson, at Calama in Chile, and at Harqua Hala in Arizona. Observations were made by Abbott and Aldrich on Mount Whitney, and by Ångström with the Smithsonian expedition to Bassour in Algeria ; and continuous observations are now carried on in several favourable localities on the American continent.

The instruments used, known as pyranometers, pyrgeometers, etc., can be calibrated by exposure in a quiet enclosure at known temperatures. When in use they are fully exposed to the sky without any shelter, and occasionally show a secondary effect from the cooling or warming of their metal supports and fittings, some forms being more affected than others. It would be interesting if comparative

* *Smithsonian Coll.*, 65, 1916.

observations could be made on them when isolated in an enclosure, evacuated as required, and with a window of rock salt, sylvine or other more or less transparent material to admit the radiation to be measured.* In the hands of Abbott, Aldrich, Ångström, Kimball and others these instruments have provided the most complete and reliable measurements of sky and solar radiation yet obtained.

TABLE I.

Days.	Hours.	Temper. of the air.	Temper. of the Actinom.	Differ.	Zenithal temp.	Mean temp. of the atmosphere.
From the 10th to the 11th of April.						
April 10.	7ʰ evening.	10·2	3·9	6·3	− 4·0	−23·5
	8	9·9	3·0	6·9	− 5·6	−25·5
	9	9·6	2·2	7·4	− 7·0	−27·0
	10	9·0	1·8	7·2	− 7·2	−27·5
... 11.	5 morning.	5·0	−3·0	8·0	−13·0	−35
	5 30′	5·0	−3·0	8·0	−13·0	−35
	6	5·5	−2·3	7·8	−12·0	−34
From the 14th to the 15th of April.						
April 14.	7ʰ evening.	8 5	0 8	7·7	− 6·0	−26
	8	7·0	−0·5	7·5	− 9·9	−30·0
	9	5·8	−1·6	7·4	−10·8	−32
	10	5 0	−2·4	7·4	−11·6	−33·5
... 15.	4 30′ morning.	1.0	−6·0	7·0	−14·7	−37·5
	5	1·0	−6·0	7·0	−14·7	−37·5
	6	1·6	−5·2	6·8	−13·7	−36·0
From the 20th to the 21st of April.						
April 20.	8ʰ evening.	5·6	−0·8	6·4	− 8·8	−29·5
	9	4·5	−2·0	6·5	−10·1	−31·5
	10	3·6	−3 0	6·6	−11·7	−33·5
... 21.	4 30′ morning.	0 0	−7·0	7·0	−15·7	−38 5
	5	0·0	−7·0	7·0	−15·7	−38·5
	5 30′	0·1	−6·5	6·6	−14·5	−37·0
From the 5th to the 6th of May.						
May 5.	5ʰ evening.	25·50	19·9	5·6	+12·9	− 2·0
	6	25·10	17·5	7 6	8·0	− 8 0
	7	23·10	15·0	8·1	4·9	−12 0
	8	22·9	13·9	9·0	2·6	−15·0
	9	21·5	12·5	9 0	1·4	−16·5
	10	17 5	10	7·5	0·6	−17 5
... 6.	4 morning.	12 1	5	7·1	−3·9	−23·5
	4 30′	12·1	5	7·1	−3·9	−23·5
	5	12	0	6·0	−1·5	−20 0
From the 23rd to the 24th of June.						
June 23.	7ʰ evening.	20·0	12 0	8 0	+2·0	−16·0
	8	17·8	10 5	7·3	1·4	−16 5
	9	17·6	10·7	6 9	"	..
	10	16·3	9·2	7·1	0 3	−18 0
... 24.	4 morning.	11·3	5 3	6·0	−2 2	−21·0
	4 30	11·5	5·6	5 9	−1·8	−20 5

The most recent sky measurements were made by Kimball, Professor of Meteorology at Washington.† The sensitive strips of his pyrgeometer were insulated in a bed of hard rubber, and exposed

* After the manner of Coblenz with the radiomicrometer. *Carnegie Institution Pubn.*, 65, App. IV.

† U.S. Dept. of Agriculture, Weather Bureau, Washington, Feb. 1918.

practically to the whole sky from zenith to horizon. The strips were in pairs, blackened and polished respectively, and the difference of temperature between the black absorbent strips and the polished reflecting ones when exposed to a source at a different temperature from that of the surrounding air (Fig. 3), was compensated by electrical energy which was carefully measured. From these results and the dimensions of the strips the radiation exchange was computed in terms of the energy that had to be supplied to equilibrate

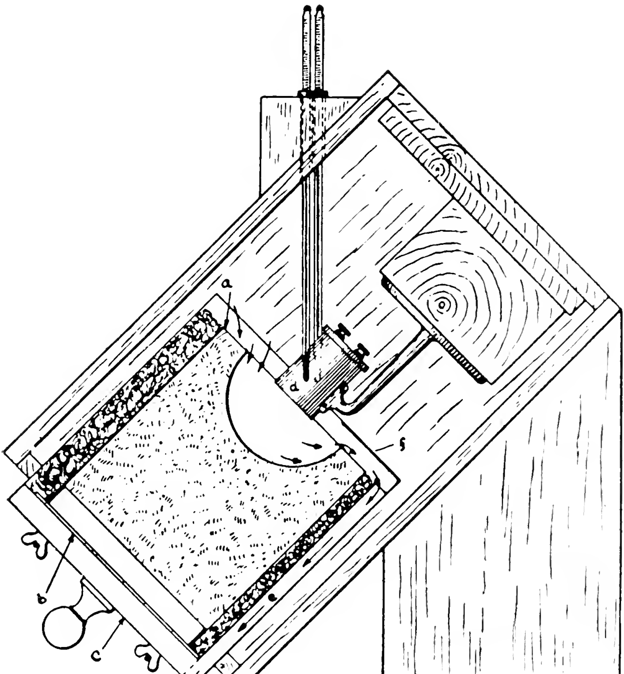

FIG. 3.

the absorbent strips, as compared with that required when black body radiation from a source at the earth temperature was employed. For night sky observations the greater this ratio the greater was the loss by radiation to the cold sky. The maximum ratio recorded was 0·437 on March 12, 1915, with the surface air just above the freezing point. Conversely, by subtracting the experimental ratio from unity, the proportionate radiation received from the sky as compared with that from the earth black body was obtained, and

hence the effective black body temperature of the sky. This on the occasion just quoted would be $-36°$ C., or a difference of temperature between earth and zenith of nearly 37°.

Following A. Ångström's methods in the Bassour expedition, Kimball also determined the amount of water vapour in the air, while observing the sky radiation. When the two sets of results were plotted, the radiation loss to the sky was seen to decrease as the amount of water vapour in the air increased, the rate of decrease being more pronounced with Kimball's results in America than with Ångström's results in Africa.

Table II. contains Kimball's mean results covering nearly a whole year, and progressing from lower to higher water vapour pressures.

TABLE II.

e	T_1	R	σT_1^4	$R/\sigma T_1^4$	ΔT
mm.	°Abs.	calories	calories		°C.
1·54	267·2	0·144	0·417	0·346	−27·0
1·82	272·9	0·175	0·454	0·385	−31·3
2·57	275·0	0·177	0·468	0·379	−30·9
3·68	280·2	0·169	0·505	0·335	−27·0
4·67	289·1	0·197	0·571	0·345	−29·1
5·31	286·7	0·175	0·554	0·317	−25·9
6·55	284·7	0·158	0·532	0·297	−23·9
7·40	288·1	0·156	0·564	0·277	−22·5
8·83	291·5	0·149	0·591	0·252	−20·4
10·99	294·9	0·134	0·619	0·217	−17·4
13·15	298·4	0·144	0·649	0·221	−18·2
15·94	297·1	0·114	0·637	0·179	−14·4

The vapour pressures e are given in the first column, with the surface temperature T_1 in the second. The third column R contains the experimental measurement of the calories supplied to equilibrate the absorbent strip. The black body radiation of the earth σT_1^4 is given in the fourth column (σ being the radiation constant of $8 \cdot 11 \times 10^{-11}$ gramme calories per minute per sq. cm.). The ratio $R/\sigma T_1^4$, given in the fifth column, indicates the proportionate loss to the sky, and if subtracted from unity gives the proportionate amount received from the sky, the "black body" at earth temperature being unity. The difference of temperature ΔT between the earth and

sky on this basis is shown in the sixth column. Fig. 4 shows the plotted relation already referred to between water vapour and nocturnal radiation. For this curve the radiation results were all reduced to an earth temperature of 20° C. to get a steady basis of comparison.

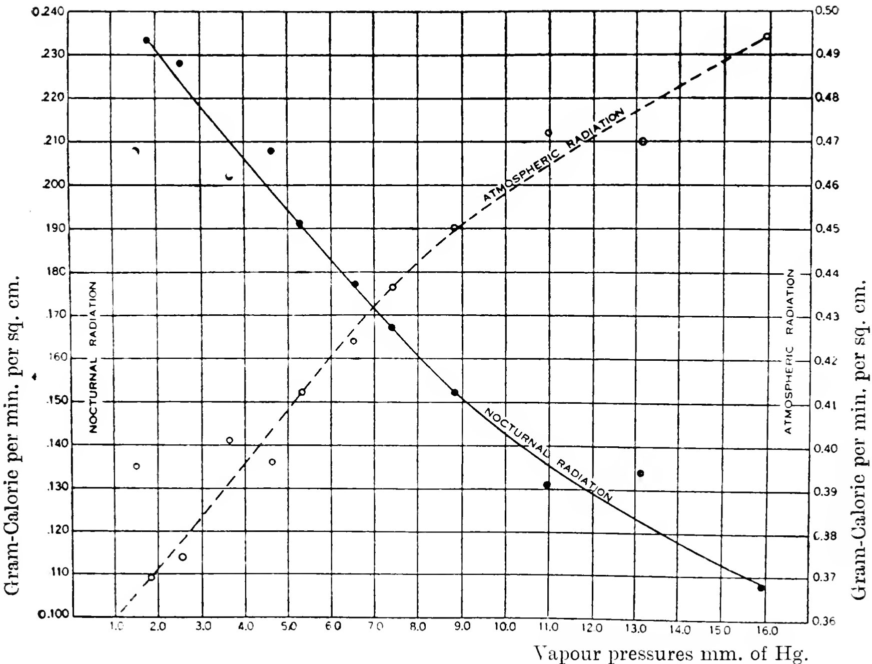

FIG. 4.

SKY RADIATION MEASUREMENTS BY CHARCOAL THERMOSCOPE.

As the charcoal thermoscope described a year ago* has proved a simple and workable instrument for comparison of radiation intensities, it was adapted to the study of the varying radiation from the sky by both day and night, and has yielded some suggestive results. The sensitive saturated charcoal surface exposed, being granular in texture, is quite a good absorber. The charcoal grains, graded to approximately $\frac{1}{2}$ mm., are the characteristic angular fragments

* *Proc. Roy. Inst.*, xxiii., p. 245, et seq.

obtained by grinding down hard carbonised coconut shell. When spread in a layer about 2 to 3 mm. thick, they present a broken cellular surface well adapted for entangling incident radiation. As already described (*loc. cit.*), 0·7 to 0·8 gramme was employed spread over a surface about $2\frac{1}{2}$ cm. in diameter. This, when enclosed in a cell under a very thin rubber membrane or rock salt window, and immersed in liquid oxygen, absorbed about 500 cc. of clean air. When it was exposed to radiation from a black surface at normal earth temperature (restricted to an axial pencil of $2\frac{1}{2}$ cm. diameter surrounded by a penumbra of diminishing intensity enclosed within a cone having an angle of approximately 25°), the displacement of the liquid index of the manometer was of the order of 3 cm. in half a minute, which corresponds to an evolution per half minute of 0·054 cc. of gas, and a utilisation of 0·0071 gramme calorie per half minute.* With the diaphragms stopped down to approximately one-hundredth of this aperture, the sun (at 3 p.m. April 8, 1921) gave a displacement of 4·4 cm., equivalent to 0·0792 cc., or 0·0104 calorie.† When the moon's image was reflected in from a silvered or platinised mirror, a distinct response was produced, in addition to that from the night sky.

The displacements resulting from exposure to the zenith varied of course considerably with the conditions, low values being recorded with clear night skies. Clear skies by day also frequently gave very low values, especially just before sunset, when the displacements were often as small as those obtained during clear nights.

The displacements of the manometer on exposure to a black dome at the temperature of the surface air and to the zenith permitted the determination of the proportion between E, the emission from the black body at earth temperature, and Z, that from the zenith, the radiations being sifted by the liquid oxygen and rubber membrane or rock salt, and absorbed into the liquid oxygen "sink" at 90° Abs. The theoretical total calories are easily deduced in the first case when

* This corresponds with the theoretical emission from the black body area exposed: viz. central uniform ring of $2\frac{1}{2}$ cm. diameter surrounded by penumbral annulus $1\frac{1}{2}$ cm. wide, diminishing to zero intensity at the edge: or the total equivalent to a uniform ring 4 cm. in diameter or 12·6 sq. cm. in area. Taking black body emission from 15° C., absorbed at 90° Abs., as 0·577 calorie per minute per sq. cm., then total from 12·6 sq. cm. area in half a minute = 3·64 calories. This total is radiated to the cell 20 cm. distant from the open neck of the flask at 15° C., and therefore is spread over a hemisphere of 20 cm. radius, or 2513 sq. cm. area; of this surface the cell of diameter $2\frac{1}{2}$ cm. occupied only 4·9 sq. cm. The proportion available to the cell is 4·9/2513 of the total of 3·64 calories emitted per half minute, or 0·0071 calorie.

† In the same way the sun effect at approximately 1/100 aperture (reflected in from silver heliostat) corresponded to a heat absorption of 0·0104 calorie per half minute, or 1·04 calorie at full aperture of 4·9 sq. cm., corresponding to 0·42 calorie per minute per sq. cm. utilised in evaporation from the partly isolated charcoal.

the earth temperature is known, and the amount of the atmospheric radiation follows from the observed ratio. The value deduced is the equivalent black body temperature of the radiating zenith. To obtain this readily, curves were drawn, based on Stefan's law, for the radiation into a liquid oxygen sink at 90° Abs. from a black body at certain definite ordinary earth temperatures. These show as ordinates the proportion to the unit black radiation at earth temperature that would be emitted by a black body at lower temperatures indicated as abscissæ. The observation ratio Z/E is then sought on the curve drawn corresponding to the known earth temperature; and from this on the scale of abscissæ the theoretical black body temperature of the observed sky radiation is obtained. Fig. 5 shows a

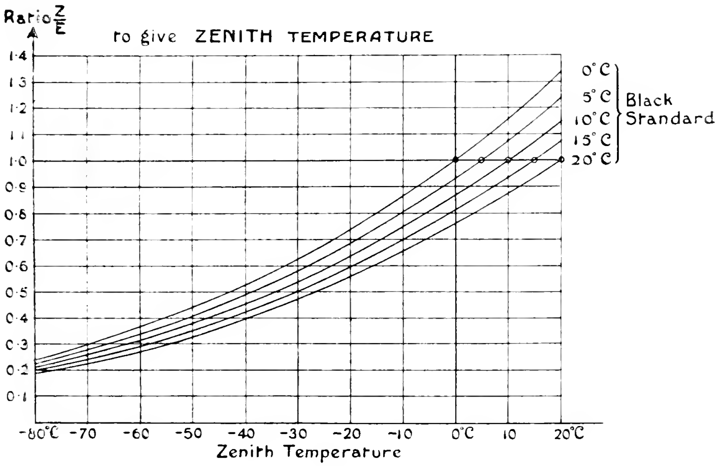

FIG. 5.

set of such curves * at 5° intervals of earth temperature from 0° C. to 20° C. As an example, with earth temperature of 15° C. and observation ratio of 0·6, the corresponding black body temperature is − 20° C. Interpolations are readily made between the curves.

Some specimens of the values obtained in this way are given in Table III. The first column gives the date and time of the observations; the second the prevailing surface temperature t; the third the observation ratio Z/E; the fourth the deduced black body zenith temperature T_Z: and the fifth the difference, Δt, between the earth and the deduced zenith temperature. No measurements of surface

* The equation of these curves is $x^4 = y\,t^4$, where $t = 0°, 5°, 10°, 15°, 20°$; $x = T_Z$; and $y = Z/E$.

Table III.

Date and Time	t	Z E	T_Z	Δt	Remarks	mm. H_2O
Mar. 7–8, 1920—	Deg. C.		Deg. C.	Deg. C.		
4 p.m. . .	8	·83	− 5	− 13	Clear afternoon sky	5·1
6 ,, . .	5	·84	− 5½	− 10½	Sunset	4·6
1 a.m. . .	1	·73	− 19	− 20	Clear stars after sleet	3·8
April 13, 1920—						
9.30 p.m. .	10	·92	+ 4½	− 5½	Low thin drift	7·0
Midnight .	9	·69	− 15½	− 24½	Clear starry night	6·3
May 10, 1920—						
9.15 p.m. .	13	·67	− 14	− 27	Some filmy stratus near horizon	6·3
10.15 ,, .	8	·563	− 30	− 38	Perfectly clear zenith: gusty	7·0
Feb. 2, 1921—						
3½ p.m. . .	5	·568	− 31	− 36	Clear, following fog in morning and preceding night. Rock-salt thermoscope	4·3
4 ,, . .	5	·487	− 40	− 45		4·3
4½ ,, . .	5	·487	− 40	− 45		4·3
6 ,, . .	5	·54	− 34	− 39		4·4
March 17, 1921—						
1 p.m. . .	10·3	·66	− 18	− 28	Clear period between cloudy times following rain	7·0
3 ,, . .	10·3	·83	− 2½	− 13	Just before becoming clouded over again	7·5
March 19, 1921—						
11 a.m. . .	12	·80	− 3	− 15	Blue zenith: some broken clouds about	5·5
March 24, 1921—						
4 p.m. . .	17½	·70	− 8	− 25½	Clear and mild	7·0
6 ,, . .	13½	·46	− 36½	− 50	Sunset; calm	6·9
6½ ,, . .	13½	·61	− 20	− 33½	After sunset	6·7
10½ p.m. to 1½ a.m.	7	·54 to ·72	− 27 to − 15	− 34 to − 22	Clear period followed by unsettled weather	5·8 to 5·5
April 12, 1921—						
3.50 p.m. .	15	·94	+ 10	− 5	Clear blue sunset sky: mild air; steady decline as darkness fell	6·5
4.30 ,, .	15	·83	+ 3½	− 11½		6·2
4.45 ,, .	15	·79	− 1½	− 16½		6·2
6.0 ,, .	13	·78	− 4½	− 17½		6·3
6.10 ,, .	13	·65	− 16½	− 29½		6·3
6.25 ,, .	13	·61	− 20½	− 33½		6·6
Midnight .	10	·66	− 16½	− 26½	Starry sky; mild	6·6
June 10, 1921—						
6½ p.m. . .	18½	·60	− 17	− 35½	High altitude wind. Cirrus, varying from thin shreds to woolly uniform pack, very high.	7·9
7¼ ,, . .	17	·74	− 5	− 22		8·4
7¾ ,, . .	16½	·50	− 28	− 44½		8·4
8 ,, . .	16	·66	− 13½	− 29½	Sunset readings; warm	8·5

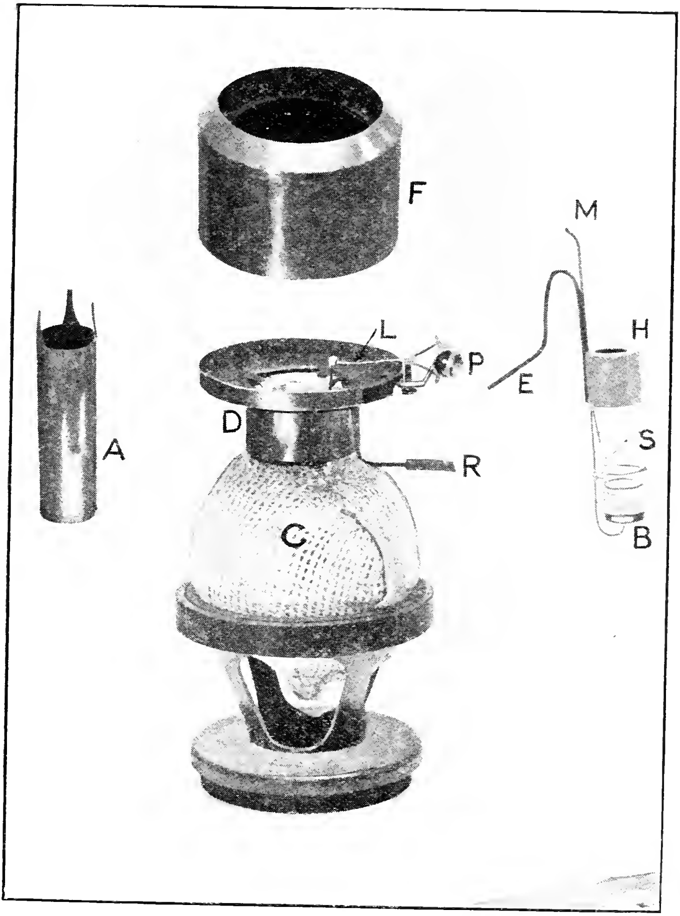

Fig. 6.

humidity were made at the place of observation, but as a rough guide to the prevailing conditions the vapour pressure values recorded in the vicinity (taken from the official British meteorological records) are noted in the last column.

From March to August, 1920, and from January to June, 1921, some 200 observations were made, mostly at night, and tabulated.

Description of Instrument.

The present form of the instrument is shown in Figs. 6 to 11. Figs. 6 and 7 showing the separate parts, and Fig. 8 a section. The thin hollow metal cylinder A carried the thermoscope cell B, immersed in liquid oxygen, all fitted into an outer $2\frac{1}{2}$-litre silvered vacuum flask C, the liquid oxygen in which was kept at a sufficient height to ensure that no variations of temperature occurred in the cylinder surrounding the cell. A was made of thin sheet copper, brass or aluminium, closed below by lead about 3 or 4 mm. thick, slightly convex to fit the curve of C; it easily passed through the flask neck, which was about $5\frac{1}{2}$ cm. in diameter and 6 cm. long, and reached to within 2 cm. of the shoulder of the flask inside. Steadiness was secured by three light springy prongs of German silver, fitting a short way up into the neck of C, a further support being provided by the tube E leading up from the thermoscope cell and bent over the neck to make a springy contact inside and out. The collar of the base plate D also fitted securely over the flask neck, and had a vertical trough into which the cell exit tube E snugly fitted. The possibility of any displacement of the cell and inner cylinder was thus reduced to a minimum. E was bent out horizontally for the short rubber connection R to the scaled index tube (*t* in Fig. 10). A sleeve of thin baize, soaked in melted paraffin, was first fitted to the neck of the flask, so that a tight fit of the metal collar and cell tube should be possible without any dangerous pressure on the glass neck. The cell B was made with a cone-jointed ring to carry the rubber membrane, or silver chloride or rock salt plate on a light lead dome, soldered to the coned ring as shown at P, in Fig. 7. The coned ring could thus be manipulated apart from the main body of B, containing the charcoal: the two parts being fitted together warm, with the joint luted by a little freshly melted rubber which rendered it air-tight at low temperatures. Ebonite can be used for the tube E and shutter connecting rod M (Fig. 8), where they pass through the neck of the flask; screwed connections are then made to the thin German silver tube and shutter hook within the flask. Here also a little melted rubber ensures an air-tight joint in the tube E within the flask. It may be mentioned that a very small leak in the cooled thermoscope is indicated by a persistent high "zero" on the instrument when all is equilibrated, the manometer

showing a continuous displacement of 1 cm. or more per minute with the shutter closed. Thin German silver may be used instead of ebonite, but there is then more conduction into the flask, and consequently slight condensation of ice on E and M within the rim of the flask when left for long periods.

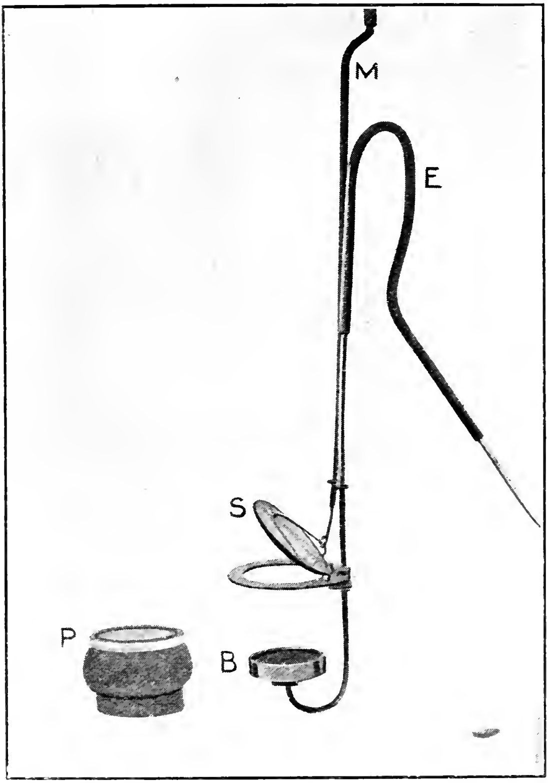

Fig. 7.

In Figs. 6 and 8, H is a light hollow copper cylinder with three equal and symmetrical diaphragms (one at each end and one in the centre), whose apertures are all $2\frac{1}{2}$ cm., equal to that of the cell. H is shown in Fig. 6 attached to the cell to give the relative position of the parts, but in use it is fitted into the neck of A after B has been adjusted (as in Fig. 8): a moderately springy sliding fit affords

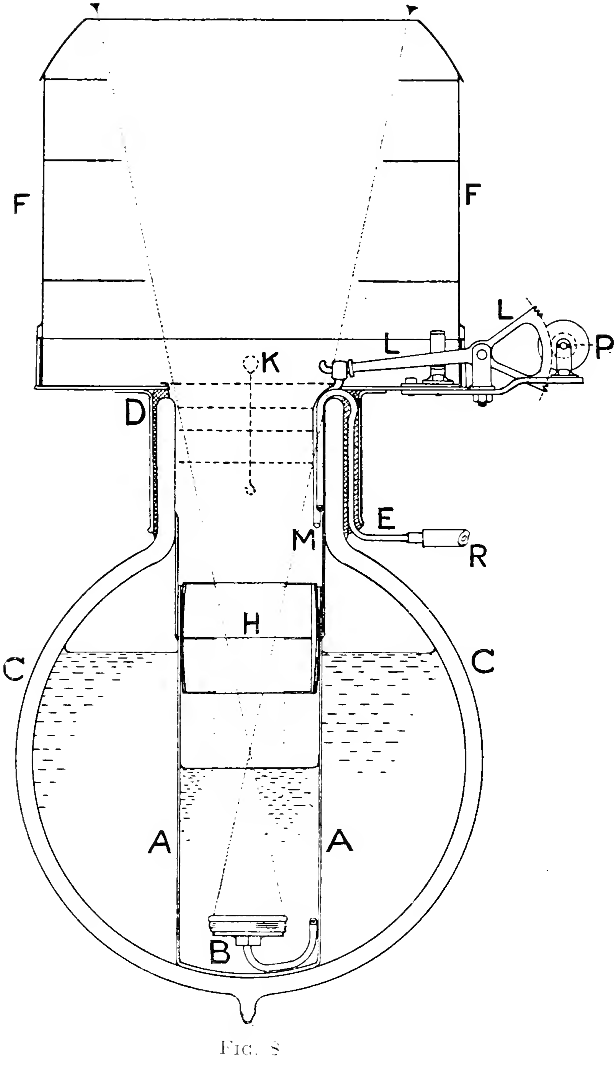

Fig. 8

sufficient security when all is cooled. The diaphragms H then take the temperature of the liquid oxygen bath, absorb all stray radiation, and limit the exposure of the cell to the zenith.

The baffle dome F (Figs. 6 and 8) provides effective protection against ordinary wind disturbance. It fits air-tight round the rim of the base plate D, and by this means the clean air arising from the slow evaporation from the flask is enclosed as in a protected pocket above the flask neck. The cylindrical shape and curved rim divert direct wind, and the baffle diaphragms (of diminishing aperture) check any eddying disturbance entering from above. Without some such device, in the open air the small pressure fluctuations transmitted through the flask neck would cause oscillations of the manometer liquid amounting to 1 cm. in four or five seconds on gusty days, while a steady wind would increase the normal "zero" reading of the instrument from about 1 mm. up to 1 cm. or more per minute.

The protection provided by F is equally necessary to prevent condensation in the flask neck and consequent contamination of the interior by ice or other condensable atmospheric matter. In the five to ten minute periods of equilibration between successive observations, F was covered by a thin metal dome. Within this, on a central light metal stage, was supported a flat lead cup packed with pieces of pumice soaked in concentrated sulphuric acid. The air inside F was thus kept pure and dry, with the result that the flask neck remained quite clean for long periods. During the intervals in which no observations were being taken, the flask neck was closed by the stopper K (Fig. 8) consisting of four or five metallic discs (polished copper or aluminium) equally spaced on a thin central rod or light tube. The uppermost metal disc was large enough to rest on the rim of the flask neck, the others passing easily inside, with a small piece cut out from each to clear the tube E and shutter connecting-rod M, which rested close inside the neck.

The liquid oxygen in A was kept quite clear by a silk gauze bag containing uranium nitrate, which was hung on the stopper-disc, and removed all traces of solid impurities by electrical attraction. The following experiment illustrates the efficacy of this device. An unsilvered vacuum vessel half full of filtered liquid air has its clear image projected. When by means of a silvered vacuum siphon dipped below the surface several expirations are bubbled through the liquid, the transparency is very rapidly replaced by a thick brown fog. A bag of uranium nitrate is then introduced and moved about in the liquid; the brown fog rapidly clears up, and in about a minute the clear transparent image is reproduced along with the black projected mass of the coated bag. When the bag is lifted out and held in the lantern beam it is seen to be thickly coated with the white solid impurities removed from the liquid air.

With the apparatus filled and settled and closed as described, the shutter could be raised without producing any effect, thus proving

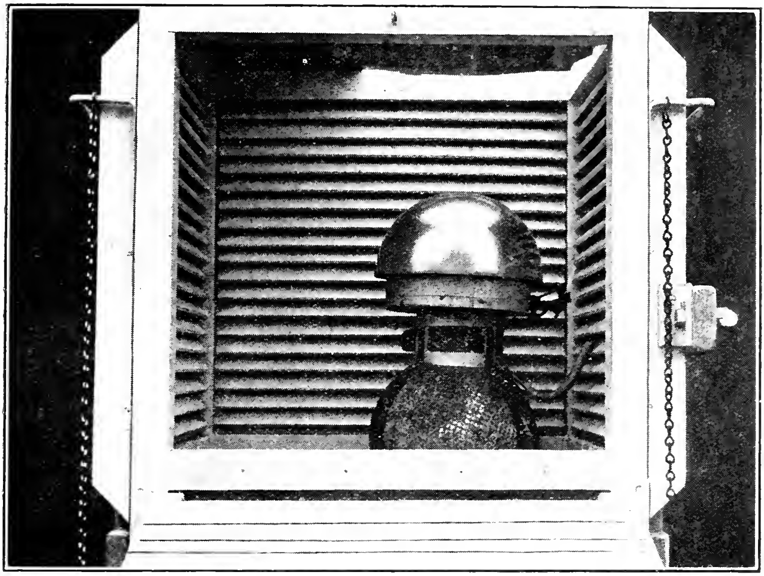

Fig. 9.

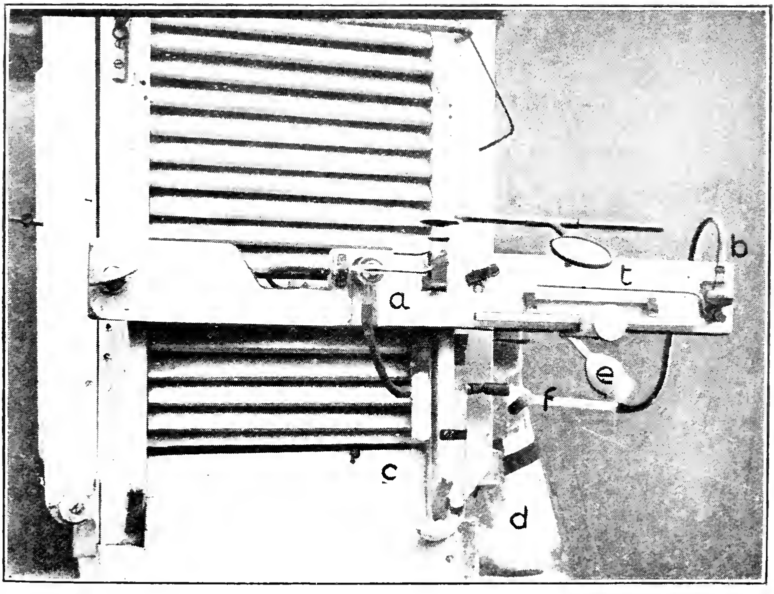

Fig. 10.

that the interior was all at a uniform low temperature. In this way the two principal sources of error were eliminated—(1) the entry of stray radiation apart from that direct from the portion of sky to be observed ; and (2) the fluctuations and contaminations arising from exposure of the instrument in the open air.

Portable Form.—A Stevenson louvred screen (as used for meteorological instruments) was utilised as a protection equally against bad weather and direct sunshine. By some simple adaptations a self-contained and easily portable instrument was evolved. In Fig. 9 one side of the Stevenson screen is seen open to show the thermoscope flask, as already described, fixed in position, stout rubber tubing passing through the slatting to the manometer, which was fixed on a horizontal pivoted batten outside, as shown in the end view (Fig. 10). The three-way cock *a* connected the cell tube either to the manometer *t* (horizontal scaled tube) or to the U-tube *c*, containing a little solid potash or granulated soda lime, and thence by a T-piece *f* to the equalising flask *d*, and also to the other end of *t*. Inserted through the rubber cork in the neck of *d* was a thistle funnel *e* bent over and lightly packed with cotton wool. Gusty fluctuations of the air were thus damped down in entering through *e* and equally transmitted to each end of the manometer, which thus remained quiet. Unsteadying pulses of pressure back to the cell charcoal were then damped out, and the cell protected from disturbance while equilibrating between readings. A thin metal hood (not shown) was screwed over the manometer and fittings to keep them dry ; and in this hood, opposite the manometer tube *t* and stopcock *a*, a hinged flap was arranged to be hooked up when observing, the sides of the screen remaining closed up. The cell shutter was then raised by turning a milled wheel with a slotted hollow spindle (in a socket in the side of the screen shown open), and engaging the pinion P (Figs. 6 and 8) which actuated the toothed quadrant lever L, for working the connecting rod M, which raised and lowered the shutter S. The details of this will be clear from Figs. 6, 7 and 8. A floor was of course added to the Stevenson screen, and four adjusting screw feet. The instrument was provided with iron handles at each end, and could thus be readily lifted complete and set in any desired position. The ordinary top provided was fitted with a thumb screw at each end, to be easily removable for observations ; the instrument was thus entirely protected, but readily available for use. There was ample space inside the Stevenson screen for a covered gas jar (containing a few pieces of fused calcium chloride under a perforated disc) in which the stopper K and attached bag of uranium nitrate were hung, when removed from the flask.

Arrangement in Laboratory.—The ordinary form of the instrument was arranged on a small gallery erected immediately under a sliding skylight in one of the laboratories of the Institution. A view

from below of this small improvised observatory is given in Fig. 11. The batten supporting the manometer was pivoted and adjustable, as in the portable form, in order to control the position of the liquid index: but no equalising flask or other similar attachment was necessary in this protected position. The actual opal scale was supported in a small rack adjustable by a pinion for easy and accurate setting, with a low-power lens arranged above. The space available was quite sufficient to allow re-filling and all necessary tests and attention without any disturbance.

The instrument was tested by exposure to a light metal vessel above, filled with alcohol cooled to known temperatures to -80° C., and with water up to $+100^\circ$ C. When it was properly adjusted the results were found to be within about 5 per cent. of the theoretical values deduced from Stefan's law, the greatest deviations being with the higher temperatures. The metal vessel for containing the cooled alcohol, etc., was conical, and the bottom concave and blackened. It fitted over the open top of F.

During the Discourse, to demonstrate the working of the thermoscope, small churn-shaped vessels, covering the open neck of the flask, were rested on the base plate D. They also were concave below (to avoid direct contact with the flask neck). The response obtained by exposure to such a vessel containing water at the temperature of the Lecture Theatre was 2·5 cm. of the projected scale in fifteen seconds. With the water at 95° to 98°, the displacement was 6·8 cm. in the same time. With the "churn" filled with boiling carbonic acid paste, 30 seconds' exposure was necessary for a displacement of 1·0 cm. When liquid oxygen replaced the carbonic acid the index was stationary. New liquid air, being about $5\frac{1}{2}^\circ$ lower, caused a small but persistent negative movement. When a current of hydrogen was passed through a capillary nozzle in this liquid air, the negative movement rapidly increased, as the lowering of temperature caused the exposed thermoscope charcoal to absorb more air, as contrasted with the previous expulsion of air when exposed to higher temperatures.

With the regular use of the instrument, the readings did not vary appreciably, even though the level of the liquid oxygen in the flask was allowed to fall several centimetres. Only when the fall was sufficient to allow a rise in temperature of the diaphragms H and the shoulder of the flask did the value begin to increase at all. The stray radiation correction thus introduced can of course be determined at any time by making a blank observation with a small plain vessel of liquid oxygen over the flask neck. The amount so determined is then deducted from all measurements to get the correct Z/E ratio, which would otherwise be too high, and as a result give deduced zenith temperatures that would not be sufficiently low. An average working condition would be with the flask from two-thirds to three-fourths full, and with up to 5 or 6 cm. depth of liquid

Fig. 11.

oxygen above the cell in the central vessel A. To maintain this condition the flask employed required the addition of about a quarter to one-third of a litre of liquid each day, preferably about an hour before observing.

Sky Observations.

Observations were made through various sequences of weather conditions, to note the resulting deduced zenith temperatures. Exceptionally low values were obtained in June and November, 1920. On the nights of June 13 and 14, the Z/E ratios at about midnight or a little later were of the order of 0·4, or even less. With the earth temperature (in London) between 13° and 15° C., the equivalent zenith temperatures were − 33° C. on the 13th, and − 47° C. on the 14th, the corresponding values of the T_Z being therefore − 46° and − 62°. The first of these observations was associated with exceptionally calm conditions, rapid temperature decrement, and sharp ground frosts at night in the open outside London, while the second values followed the clearing of the atmosphere by a thunderstorm. Beyond these exceptionally low records, the Z/E ratio only occasionally fell to 0·5, with a T_Z of the order of − 40° C.; but a good clear sky quite frequently gave a ratio round about 0·6. From sunset to past midnight on Sundays low values were not unusual, possibly associated with the clearing of the air over London as the traffic subsided in the week-end lull. On several occasions a clear atmosphere following heavy rain also gave low ratios. Readings taken before sunset were as a rule lower than those just after, and frequently quite as low as the midnight values. The increase in the ratio at sunrise was very sharply marked, but only occasionally did it rise above unity when the sky remained clear. Thus, on May 4, 1920, the Z/E ratio just before dawn (the observation was made at Mill Hill, ten miles out of London) was 0·63, with the black standard (in this case a strip of velvet) at + 0·5° C., giving a T_Z of − 29½° C. Air temperature just above grass was − 2·6° C., and in the grass − 1·5° C. Half an hour later with sufficient dawn light to read the scale (bird life just stirring), water was dripping from the thermometer above the grass which read 0·5° C., the other thermometer being unchanged. The Z/E ratio was then 0·72, corresponding to a T_Z of − 22° C.

As already stated, during daylight the values of the Z/E ratio were frequently below unity, apart from the more common low values already noted that preceded sunset. A clear blue zenith was of course associated with such readings, while cloudless haze or sunlit cloud gave values equal to or greater than unity. The first occasion on which a low daylight ratio was recorded was with a deep clear blue zenith above brilliant white flecks of clouds extending about 60°

above the horizon. This was three-quarters of an hour after noon on May 9, 1920, following a night of drizzling rain with steadily improving conditions during the morning: air temperature was 16° C. in the shade (22° C. in the sun), and Z/E was 0·68, corresponding to T_Z of − 8° C. With one of the small white clouds included in the zone of exposure, the ratio increased to 0·78. Low daylight values were also observed on several occasions in March and April, 1921, generally with short intervals of clear blue sky between showery periods associated with blue sky and white broken clouds. The ratios then were frequently of the order of 0·7, and sometimes a little below.

Typical determinations of sky radiation, such as those made at Hump Mountain, North Carolina, and also at Calama, Chile, by Moore and Abbott with their electrical pyranometers, showed that the radiation from the whole sky when very clear may be as low as one-fourteenth that of the sun, rising with cloudy skies to as much as 60 per cent. of that normally due to the sun alone.* The same observers also found lower values from the upper than from the lower zones of the sky, but on no occasion was there actual loss of radiation to a daylight sky, however clear.

There are of course several absorption bands in the visible transmission spectrum of liquid oxygen † which suggests the possibility of appreciable absorption from the daylight sky; but careful measurements would be necessary to define its amount, and on the other hand, the charcoal thermoscope responds strongly to the radiation from sunlit clouds, even when a cooled glass screen is interposed. The apparent loss of earth radiation to clear daylight zenith, as indicated by the charcoal thermoscope, may therefore be regarded as sufficiently definite to provoke further enquiry.

Absorption by Atmospheric Constituents.

The increase observed in the Z/E ratio just after sunset and sunrise would be partly accounted for by the temporary increase of water vapour content in the surface air by condensation in the first case and evaporation in the second. This absorption by water vapour at various wave lengths has been extensively studied by Fowle and Aldrich.‡ A sensitive bolometer measured the energy in different parts of the heat spectrum as distributed by a rock-salt prism, after passage through a chamber over 100 metres in length. By long-focus mirrors the path could be greatly extended. The

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* *Annals Astr. Phys. Laby.*, vol. iv. p. 259, et seq., Measures of Brightness of the Sky, etc. etc.

† *Proc. Roy. Inst.*, xv. p. 556.

‡ *Ann. Smithsonian Astrophysical Observatory*, iv. 27 et seq.

proportions of the absorbable constituents (water vapour, carbon dioxide and ozone) in the atmosphere of the chamber were controlled, and the resulting alterations of intensity at all parts of the analysed beam determined by the bolometer. The measurements up to wave lengths of over 50 μ were finally extended with extraordinary precautions to the atmospheric absorption of the sun's radiation.

Lord Rayleigh, in a Discourse given in the Royal Institution on May 7, 1920, gave an account of the absorption of the short ultraviolet solar radiation by the thin layer of ozone in the upper air. The absorption of ozone for the long wave lengths has been measured by the charcoal thermoscope, the same apparatus as was employed for the zenith observations being used. A dilute solution of liquid ozone in liquid oxygen was placed in the central vessel A above the thermoscope cell, the ozone solution being obtained by bubbling a slow stream of ozonised oxygen into A. The diaphragm drum H was removed, and the liquid was kept clear of solid impurity by an electrified quartz tube. The displacements obtained by exposure to the black source at room temperature were determined before and after the condensation of liquid ozone in the liquid oxygen. The percentage of ozone was afterwards determined by decanting out a small measured quantity of the ozone solution in A and evaporating it through potassium iodide. The observations had to be made with the least possible delay, as even the very dilute ozone solution employed soon attacked the rubber membrane. A succession of crackling explosions occurred, which resulted each time in a loss of concentration of the ozone, and finally the membrane was punctured. A rock-salt cell would no doubt have been preferable, but was not available at the time.

Similar measurements with more concentrated ozone, or better still with pure liquid ozone, would be interesting. For more concentrated solutions a separate vacuum vessel would be desirable, the radiation to be measured being reflected in and out again by double silver mirrors, before being finally reflected into the thermoscope. This arrangement was tried with ordinary clean liquid oxygen and found to be quite workable. The results obtained with the dilute solution of ozone as described showed that 5 cm. depth of an ozone-oxygen liquid, containing only 0·18 gramme of ozone in the total 88 grammes of liquid oxygen, reduced the manometer reading by 16 per cent. on exposure to the same black source as the oxygen at 19·7° C., the values being 6·0 cm. in half a minute with pure oxygen, and 5·25 to 5·35 cm. in the same time with the 1 in 489 dilution of ozone-oxygen liquid. By volume the proportion would be 1 of ozone to 730 of oxygen, so that the actual thickness of pure ozone liquid if separated would be only 0·07 mm. Fowle and Aldrich concluded that the ozone in the upper air cuts off one-fifth of the outgoing radiation of the earth which has been transmitted through the lower layers of the atmosphere. To the quality of

radiation affected—from $8\frac{1}{2}\,\mu$ to $14\,\mu$—all the other constituents in the atmosphere appear very transparent. The functions of the ozone in our atmosphere would thus seem to be very extensive.

The carbonic acid absorption for long waves was also determined by the Smithsonian experimenters (*loc. cit.*), who were able to identify an absorptive region beyond a wave length of $13\,\mu$. Carbonic acid being present in the atmosphere in smaller proportion than water vapour, its absorption is less important. Since the atmosphere weighs approximately one kg. per sq. cm. the solid layer of carbonic acid snow (4/10,000 by volume, or 6·1/10,000 by weight) would be 6/10ths of a gramme per sq. cm., or only about 4 mm. thick all over the earth's surface.

The distribution of the principal constituents of the atmosphere has been studied by Cailletet, Muntz and Schloesing, who obtained and analysed samples from a height of $15\frac{1}{2}$ km. They found no variation of the proportions up to that height. An adaptation of the high efficiency rockets devised by Goddard * would, however, make it possible to secure samples from much greater heights. Table IV. is a list of the substances present in the air. The final absorptive effect as estimated by Fowle and Aldrich (*loc. cit.*) is such that only 20 per cent. of the earth's radiation escapes into space.

Table IV.—Substances found in the Air.

—	Mol. Weight
Hydrogen and helium	2 and 4
Water, marsh gas, ammonia, neon	16 to 20
Nitrogen, oxygen, carbonic oxide, hydrogen peroxide	28 ,, 34
Argon, carbonic acid, ozone, nitrous and nitric acids	40 ,, 53
Sulphurous acid	64
Krypton and xenon	88 and 128
Radium emanation	222
Inorganic dust (factories and volcanoes)	—
Organic effusions (natural and industrial)	—
Spores and microbes	—
Meteoric dust, etc.	—

Cloud Radiation.

The earliest charcoal thermoscope was actuated by a beam whose radiation was sifted from most of the long wave lengths by passing through the glass bulb at liquid air temperature. When the present thermoscope therefore is exposed to luminous clouds higher readings

* *Smithsonian Coll.*, 1920.

are obtained than from dark clouds. A bright cloud in early June gave 2·2 times the black dome displacement at earth temperature, while even in February a Z/E ratio of 1·6 was obtained. The results were generally higher at or just before noon than a few hours later (direct sunlight, if any, being screened off). The proportion due to short wave lengths, excluding those known to be absorbed by liquid air,* was investigated by filtering the radiation through cooled glass (as for example, a small beaker containing 2 or 3 cm. depth of clean liquid oxygen, in the neck of the thermoscope flask, or a thin

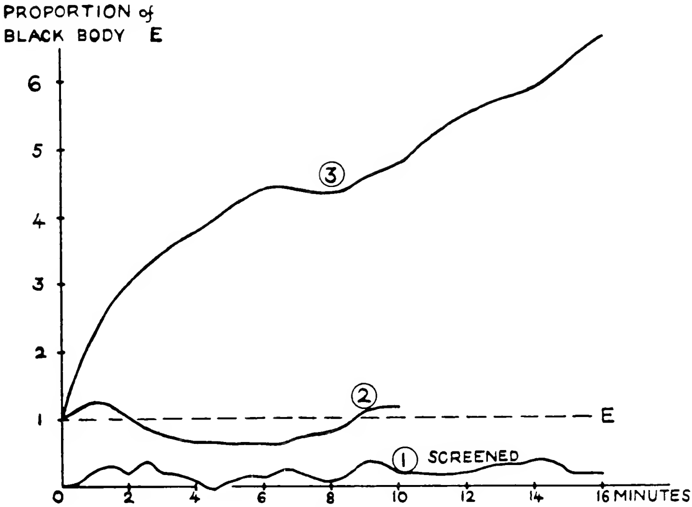

Fig. 12.

glass disc in a hinged ring covering when required the aperture of the lowest diaphragm above the cell shutter).

The responses obtained when continuous exposures with the cold glass screen were made while variable broken clouds crossed the zenith (on July 13, earth temperature 23·7° C.) are shown in curve 1 of Fig. 12, the scale of ordinates being in terms of unity taken for the value given by the black dome at the time. The passage of bright portions of cloud corresponded to the peaks of the curve, while values near or below zero (negative reading) resulted from the clearer blue spaces. Corresponding curves from observations without any cold glass screen are shown in curves 2 and 3 (taken March 21,

* *Proc. Roy. Inst.*, xv. p. 556.

earth temperature 10° C.). They show values below unity for clear skies (curve 2), and much greater than unity for bright clouds (curve 3). Exposure was first made to the black dome until no further displacement was occurring (about 12 minutes), the thermoscope cell being then in equilibrium with the black body at 10° C. The dome was then removed to expose the cell to the zenith; the continuous displacements that resulted are therefore in excess of the black readings. The flat portion of curve 3 from the 6th to the 8th minute resulted from a grey patch in the cloud mass. With the succeeding brighter parts the irregular rise in the manometer continued, and after the 16th minute was still very rapid, whereas after 12 minutes' exposure to the black dome, the manometer became stationary. The interposition of the hand at this moment above the aperture of the instrument produced an immediate negative response (backward movement of the manometer). The positive and negative fluctuations in curve 2, when exposure was made to a fairly clear sky, correspond respectively to bright flecks of cloud, or a clear blue zenith.

Dull or black clouds almost invariably gave values equal to the earth temperature black body. On one or two occasions, however, warm cloud layers were registered, as for example when a low dark cloud gave a reading 1·48 times the black at 6° C., which would correspond to a black temperature of 35° C.; this was registered on both the rubber membrane and rock-salt thermoscopes on February 10, together with several low values with clear skies. Cold cloud banks were not uncommon: on one occasion a dense uniform layer gave a reading corresponding to a temperature of $-7\frac{1}{2}$° C., with the earth temperature at 10° C. (4.45 p.m., January 22). Black fog banks registered a lower temperature than the earth: for example, on December 8 a ratio of 0·82 was registered, corresponding to a temperature of − 4° C. The approach to the zenith of a dense black cloud, from which hail fell almost immediately afterwards, gave a Z/E ratio of 0·85, or a temperature a little below 2° C., the earth black body being at 14·5° C.; had it been possible to expose with this cloud occupying the whole zenith, a lower value would doubtless have resulted (9.30 a.m. May 4, 1920).

Demonstration of Cloud Formations.

The following experiments were shown during the Discourse to illustrate some aspects of the texture of clouds formed by rapid cooling.

A comparison between light and heavy clouds was made by pouring some 100 cc. of liquid air into a cooled cylindrical vacuum vessel having a spiral draining tube sealed in below (Fig. 13). To this outlet the compressed air circuit was connected, and a rapid

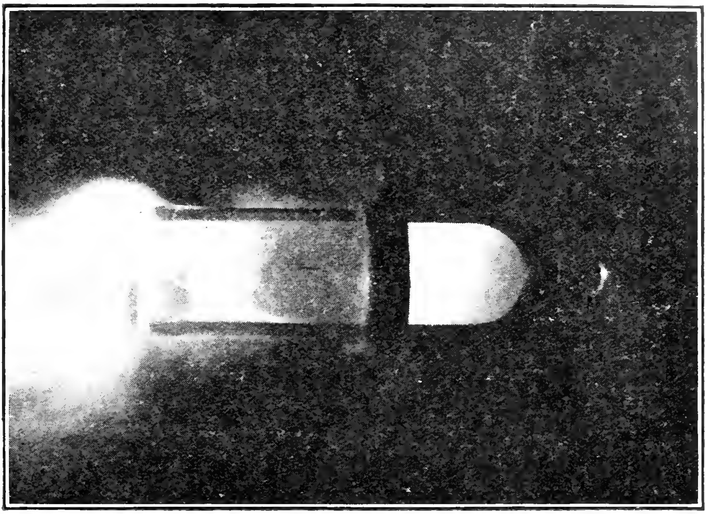

FIG. 14.

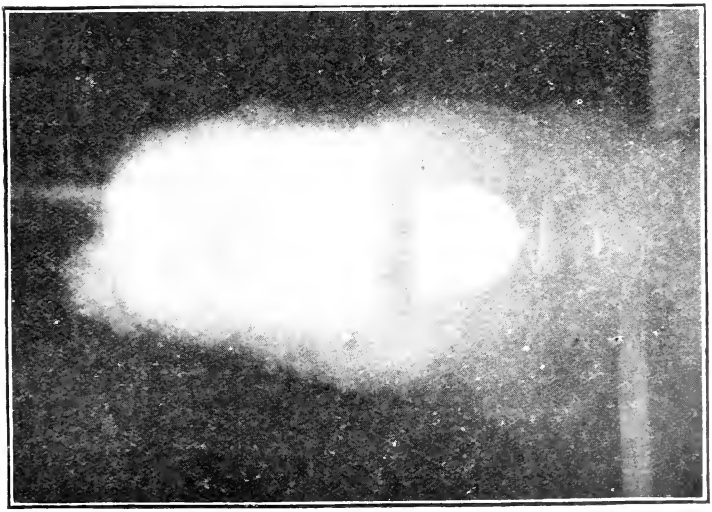

FIG. 13.

current of air first established. On liquid air being poured in, heavy clouds flowed out and immediately rolled down the outside, almost obscuring the vessel. On repeating this with a similar current of hydrogen, the cold clouds were rendered quite buoyant and soared away with only a small amount of aggregation, as seen in the companion Fig. 14.

Slow surging movements and cirrus-like formations were shown by illuminating the quiet layer of cloud over a vacuum cup of liquid air. For this purpose a slightly convergent beam was passed through

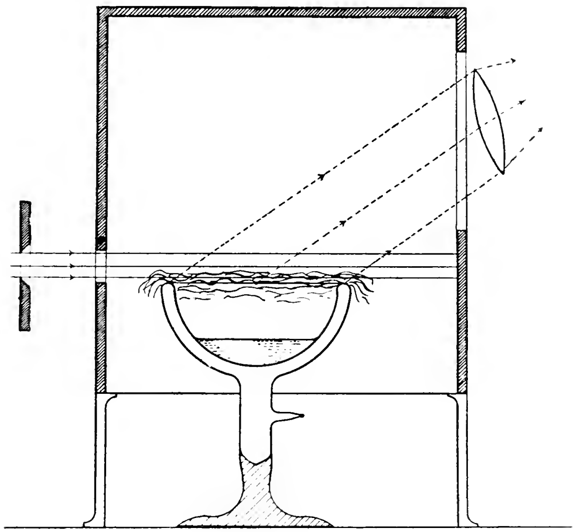

FIG. 15.

a diaphragm with a horizontal slit, and glanced along the open mouth of the cup. All stray light was absorbed when the cup, wrapped with black velvet, was placed inside a black box having a horizontal slit in one side at the same height as the mouth of the cup. The illuminated clouds were then projected through an elliptical opening higher up on the side of the box opposite to the incoming light. Fig. 15 shows the arrangement, and Figs. 16–20 some of the formations observed.

On a larger scale several beautiful effects were seen when small quantities of liquid air were distributed over the surface of a blackened

shallow vessel of slightly tepid water. At first single drops of liquid air were allowed to fall from a height of 35 feet from a dropping pipette.* Each drop produced a little whirling cloud as it wandered about in the spheroidal state on the surface of the water. The rate of dropping was then increased to a rain, which soon obscured most of the water surface under a misty mantle. A dense rolling cloud mass was then obtained by tossing spoonfuls of the liquid air about all over the vessel. With a localised spot-light illumination all the miniature alpine valley effects were seen very distinctly.

A more widely distributed cloud was produced by a whirling inverted metal cup fixed on the vertical spindle of a small motor, and supported a few inches above the surface of liquid air in a squat metal cylindrical vacuum vessel. The cup was divided by radial plates into eight sectors, but with a small centre space left clear.

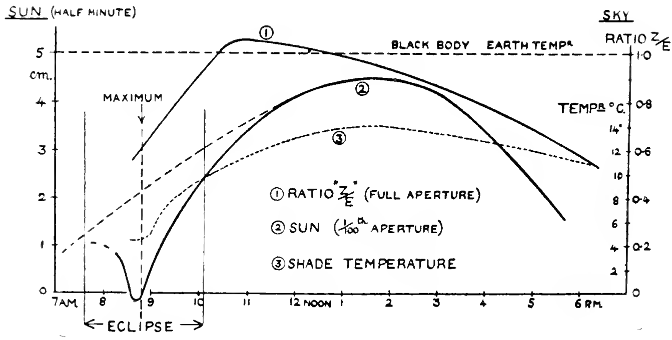

FIG. 21.

About a litre of liquid air was poured into the vacuum cylinder, and the motor started. As the speed was continuously increased, a waterspout of liquid air was steadily lifted and finally projected out into the room to form a thick cloud mantle several feet across.

Solar and Lunar Measurements.

A series of measurements were made during the partial solar eclipse of April 8, 1921. A silvered-surface heliostat was employed with a platinised mirror above the portable thermoscope, all arranged on the laboratory roof some 80 feet above the ground. A well-defined decrease and increase in the solar radiation was recorded, using 1/100 of the full aperture employed for zenith measurements.

* *Proc. Roy. Inst.*, xx. pp. 586–7.

Fig. 16.

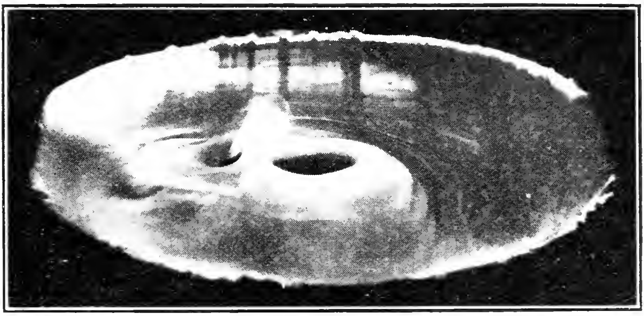

Fig. 17.

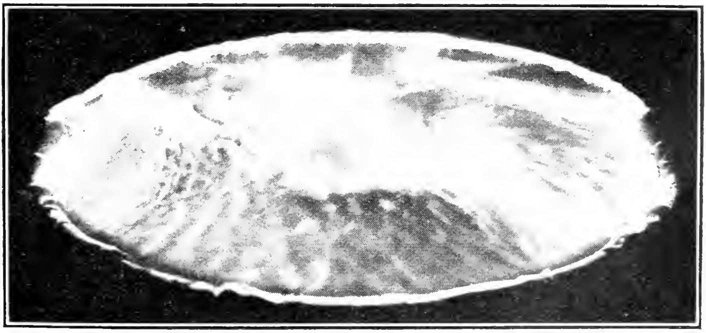

Fig. 18.

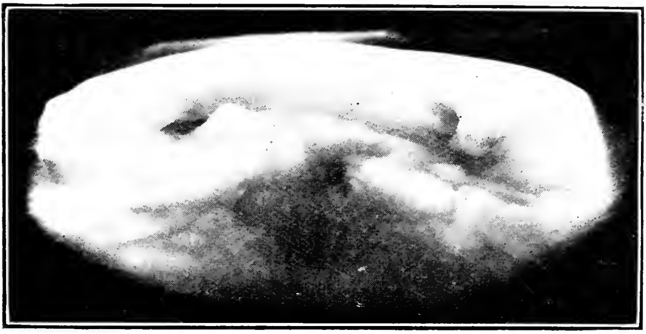

Fig. 19.

Fig. 20.

The observations were continued throughout the day; these and the corresponding values of the Z/E ratio are shown in Fig. 21. The sun displacements are given directly in terms of the readings of the cm. scale, the displacement due to the black body at 15° C. (full aperture) being 3·0 ± 0·1 cm. The shade temperatures are also included. No definite clouds were visible, but a thin persistent drift could easily be detected when the sky was viewed through an amber screen. This was no doubt partly the London smoke haze.

Similar attempts were made at the time of the total lunar eclipse of May 2–3, 1920. The instrument was, however, then in a more crude condition, and the weather was largely overcast until the eclipsed moon had sunk very low. Succeeding nights were, however, clear, and some comparative measurements were made with and without the full moon in the same aperture of the instrument by directing the mirror to different regions of the sky at the same elevation. The instrument was set up a few feet above the ground in an open meadow some ten miles N.W. from London. A clump of trees obscured up to 15° of the sky above the horizon from the east to the south, while some houses 50 yards away extending from S.W. to W. obscured about two-thirds of this amount. The manometer readings for half a minute were as follows, the effective aperture of the instrument being less than 20°:—

(1) May 2–3—

Midnight	Black body at 8° C.	3·0 cm.	
11.30 p.m.	Zenith (clear)	1·8 cm.	
11 p.m.	Sky zone with full moon	2·6 cm.	
2.15 a.m.	Ditto with moon eclipsed	2·25 cm.	

(2) May 4—

11.15 to 11.45 p.m.	Black body at 2° C.	2·4 cm.	
	Zenith (hazy)	1·9 cm.	
	Sky zone with full moon	2·95 cm.	at same elevation
	Ditto without moon, 30° S. of above	2·50 cm.	
	Ditto without moon, to N.NE. (opposite)	2·0 cm.	

(3) May 5—

12.30 to 1.15 a.m.	Black body at 0° C.	2·9 cm.	
	Zenith (clear stars)	2·15 cm.	
	Sky zone with full moon	2·95 cm.	at same elevation
	Ditto without moon, due S.	2·25 cm.	

From (1) it follows that the additional radiation due to the full moon was $\frac{2\cdot6 - 2\cdot25}{2\cdot25}$ of the value for the sky zone alone, or $\frac{0\cdot35}{2\cdot25} \times \frac{2\cdot25}{3} = 0\cdot116$ of the black body at 8° C.

From (2) it follows that at about 11.30 p.m. the additional radiation due to the full moon was $\frac{0\cdot45}{2\cdot50}$ of the value for the sky zone alone, or $\frac{0\cdot45}{2\cdot50} \times \frac{2\cdot50}{2\cdot40} = 0\cdot1875$ of the black body at 2° C.

From (3) it follows that at 1 a.m. the additional radiation due to the full moon was $\frac{0\cdot70}{2\cdot25}$ of the sky zone alone; but as the sky was then $\frac{2\cdot25}{2\cdot90}$ of black at 0° C. (equivalent to a black body temperature of $-$ 17° C., see curve of Fig. 5), the moon radiation was $\frac{0\cdot70}{2\cdot25} \times \frac{2\cdot25}{2\cdot90} =$ 0·241 of the black at 0° C.

Now it has been shown (p. 420) that the radiation available to the thermoscope of 4·9 sq. cm. area per half minute from black at 15° C. is of the order of 0·0071 calorie. This would be 0·0064 calorie from black at 8° C.; 0·0059 calorie from black at 2° C.: and 0·0057 calorie from black at 0° C. The moon's emission per minute into an area of 4·9 sq. cm. would thus appear to be

from (1) $2 \times 0\cdot116 \times 0\cdot0064 = 0\cdot00148$ calorie
from (2) (11.30 p.m) $2 \times 0\cdot1875 \times 0\cdot0059 = 0\cdot00221$ calorie
from (3) (1 a.m.) $2 \times 0\cdot241 \times 0\cdot0057 = 0\cdot00275$ calorie.

In terms of calories per minute per sq. cm. of absorbing surface the moon's emission is thus 0·0003 from (1), 0·00045 from (2) and 0·00056 from (3). The value deduced from (1) is of less significance than those obtained from (2) and (3) owing to the three hours' interval between the sky and moon observations.

Since the luminous emission per unit area from the moon is only 1/400,000 of that of the sun, it must be admitted that these values appear to be very high. As already stated, the construction of the instrument at the time was very imperfectly developed, and the results are probably therefore only directional.

Pressure Increase by Radiation into Charcoal at Low Gas Pressures.

The response due to radiation into gas-charged charcoal can be measured not only at atmospheric pressure as already described, but down to pressures of less than one-millionth of an atmosphere.

These small gas pressures were measured on a Macleod gauge to which was sealed a bulb containing 1 gramme of charcoal. After initial evacuation to below 0·0001 mm. with the charcoal at 300° C., the bulb was cooled in liquid air. Small measured volumes of pure air, nitrogen and hydrogen respectively were then admitted. The

equilibration pressures so obtained did not exceed 0·005 mm. The experiments were made in a dark room, and the charcoal bulb was cooled in a glass vacuum vessel enclosed in a black screen. A slit and shutter allowed the light from a 45-watt tungsten lamp to be

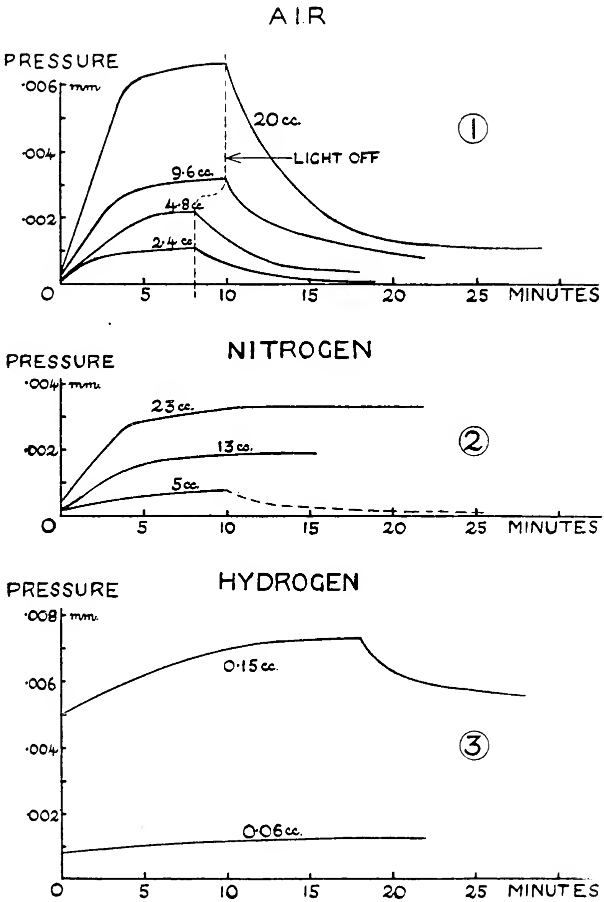

FIG. 22.

focussed on the charcoal bulb from a distance of 1 metre. Increases of pressure were observed in all cases, reaching their maximum in about five minutes with air and nitrogen, and disappearing in about the same time when the shutter was closed. The volumes admitted

and the pressures observed, with and without the light excitation, are set out in the following tables ; and the manner of the rise and fall of pressure with time is shown in Fig. 22. The increments of pressure, though small, are obviously sufficient to cause some loss of efficiency of charcoal-vacuum apparatus subjected to strong light.

Absorbed Gas	Volume	Initial Pressure (dark)	Pressure Increment with Light	Tempera-ture
	c.c.	mm. Hg.	mm. Hg.	Deg. Abs.
(1) Air . . .	2·4	·0001	·00095	83½
	4·8	·00022	·00186	83½
	9·6	·00039	·0028	83½
	9·6	·0006	·0047	85
	20·0	·00085	·0057	83½
(2) Nitrogen pure ; free from H. . .	5·0	·00016	·0006	83½
	13·0	·00024	·0016	83½
	23·0	·0004	·0029	83½
(3) Hydrogen . .	0·06	·00078	·00054	85
	0·15	·0050	·0023	85
	1·2	·0316	·0032	84½

Acknowledgment is due to Mr. W. J. Green, B.Sc., for the valuable assistance he rendered in the laboratory and in the preparation of this account of the work.

December, 1923.

WEEKLY EVENING MEETING,

Friday, January 21, 1921.

The Hon. Sir Charles Parsons, K.C.B. LL.D. F.R.S, Vice-President, in the Chair.

Sir Frank Benson.

Shakespeare and Democracy.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, June 17, 1921.

His Grace the Duke of Northumberland, C.B.E., M.V.O., President, in the Chair.

Sir J. J. Thomson, O.M. LL.D. D.Sc. F.R.S., Master of Trinity, Honorary Professor of Natural Philosophy, R.I.

Chemical Combination and the Structure of the Molecule.

[No Abstract.]

GOLDEN WEDDING PRESENTATION TO SIR JAMES AND LADY DEWAR.

After the Discourse the Chairman said :—

Ladies and gentlemen, I have a very pleasing duty to perform this evening. As you probably know, Sir James and Lady Dewar are celebrating their golden wedding next August, and this, the concluding lecture of the season, was felt to be the most suitable occasion for presenting the gift which I now have the honour to present from this Institution to Sir James and Lady Dewar in recognition of the great regard, esteem and affection in which we all hold them. I feel that it would be almost an impertinence on my part to speak to such an assembly as this of the great services which Sir James Dewar has rendered, not only to this Institution, but to British science, and indeed to civilisation. His name is not only a household word with all of us, but it is really no exaggeration to say that every one of us utilises in some way or another the result of the great discoveries which he has made during the last forty to fifty years.

But although it is unnecessary to speak to you of his services, I think you will agree that it is, perhaps, fitting on this occasion that we should make a brief review of the past forty-four years, during which Sir James Dewar has been Fullerian Professor at this Institution, in order that we may remind ourselves of the very great obligations which we owe him. It was in 1877, forty-four years ago, that he came to this Institution, and directly afterwards he turned his attention to the subject of the physiology of the eye, and began, in conjunction with Professor Liveing, that long series of

spectroscopic researches which has made both their names so justly famous.

A few years later—in 1888—he was appointed a member of the Committee on Explosives, and with the late Sir Frederick Abel invented the explosive cordite which the British army and navy have used ever since, and which they used in the greatest war in history, and to which our victory in that war was largely due.

But perhaps Sir James Dewar's greatest service has been the preparation and handling of liquid air in large quantities, thus making it available for experiment. For this purpose, in 1892 he devised the vacuum-jacketed vessel, which was afterwards known as the thermos flask, and has proved so very useful to all of us. Then in 1898 came the further great step of obtaining liquid hydrogen in large volumes. On one occasion 5 litres was carried from this Institution to the Royal Society at Burlington House. What this meant may be imagined if we bear in mind that the boiling point of hydrogen is 422° below zero Fahrenheit.

The result of these great researches has been an immense benefit, as I say, to civilisation. Liquid oxygen is now a commercial article, and to prove its value we have only to look at the attempt which is now being made to ascend Mount Everest, an attempt which would be absolutely impossible if it were not for that invention. If that attempt succeeds it will be due, not only to the skill, experience and intrepidity of the explorers, but to the inventive genius of Sir James Dewar.

But perhaps his labours can best be judged by my giving you a few figures. In the course of the last forty-four years he has delivered more than fifty Friday evening discourses; he has delivered more than thirty sets of lectures covering the whole range of chemistry and chemico-physics; he has delivered nine sets of Christmas lectures to juveniles, and has thus firmly established in the minds of the rising generation a foundation of scientific study.

I am sure you will agree that he has not only very worthily maintained the traditions of this Institution, but has enhanced them and has followed worthily in the footsteps of those great men that we all honour so much—Davy, Young, Faraday, and a host of others. It is all very well for Englishmen to praise our great scientists, but when praise comes from foreigners it is doubly valuable, and I should like to read what Dr. George E. Hale of America has said about Sir James Dewar, because it puts in the smallest possible compass and in the neatest possible way what I would like to say myself, but can never hope to express so happily. In a letter received two months ago, Dr. Hale, who is Director of the Mount Wilson Solar Observatory, says: "For many years I have been an ardent admirer of the Royal Institution, to which I return on every possible occasion. In some mysterious way, which the conservators of other scientific establishments might envy, the managers and the pro-

fessors of chemistry succeed beyond others in retaining and rendering tangible the very atmosphere of research bequeathed to them by Rumford, Young and Davy, and no other laboratory, however grand, can produce so potently the direct inspiration of the masters of the past."

I feel sure that I should be misinterpreting your sentiments if I gave the impression that by this gift we merely wished to show our esteem for the great services which Sir James Dewar has rendered. We want to express something much more than that. We want to express our affection both for him and for Lady Dewar. I think it is true to say that as his researches brought him lower and lower in the scale of temperature, into a more and more frigid atmosphere, so our feelings towards him have grown steadily warmer and warmer.

I have said a good deal about Sir James Dewar's services, but I have as yet said nothing at all about Lady Dewar's. We do not know, of course, how much Sir James Dewar owes to Lady Dewar; that is for him to say; but I am quite sure that he will agree that these achievements, some of which I have chronicled, though only a small part of them, are due in a very large measure to Lady Dewar, and that without her they would never have been done. Although we do not know what he owes to her, we do know what we owe to her, and I am sure you will agree that a great part of the pleasure which we have derived from these Friday evening discourses has been due to Lady Dewar's kind hospitality, and the very charming and gracious manner in which she has dispensed it.

Sir James and Lady Dewar, I now have the pleasure and the honour to present to you, in the name of this Institution, this gift, in recognition of the great services you have both rendered to the Institution, and of the great affection and regard which we have for both of you.

Sir James Dewar (who was received with cheers):—My Lord Duke, Ladies and Gentlemen, it is impossible for me, and I am sure it is equally impossible for my wife, to convey to you how surprised we were when we learnt that the members of the Institution, in anticipation of our lives extending to the month of August, wished to commemorate our Golden Wedding. Certainly I have to acknowledge that there has been no impediment during the harmonious life we have spent. It may be said at once, to satisfy the Duke, that the services of Lady Dewar have been absolutely essential. It was a match not connected at all with science. That is the proper way to begin.

The connection on her side was a heredity of art; mine was sporadic art, and that sporadic art was the idea of being a musician. Early vanity impelled me to make my own music, which was a little insane. A few weeks ago, when I received this same kind of recognition in another way, not in the rich golden cup I see before me,

but from my kind chemical friends, I told the story of my beginning to be a Stradivarius. I said I was essentially a product of disease. I think I was. In those days I could do nothing else. Consequently I laboured to copy Stradivarius, with the idea of producing an instrument on which I might learn to play, because up to that time I had only been able to learn to play the flute. I did not know what I was going to meet when I commenced to try to play the violin; but in any case the cart came before the horse, and in neither case was it a success.

I have discovered within the last few weeks that my wife had the violin secreted in Cambridge, and I have had it repaired so that it can be examined. You will note the vanity of the maker, because it is duly signed—my first authentic signature—"James Dewar, 1854." I imitated the Italian makers by signing my name, evidently in order that it might not be forgotten. Now that it has been repaired, I hope that when your Grace comes to the professorial rooms to-night, as friends are in the habit of doing, you will see the instrument; but I am not going to pretend now to have retained any of my qualities as an executant. Sir Alexander Mackenzie has kindly sent two young ladies who will at any rate let you hear that there is a *chant du bois* in it. This was the first experiment I made, and it was one which taught me manipulative dexterity—the co-ordination of hand and brain—which has been of the greatest use to me in my scientific career.

With regard to what your Grace has said of my achievements in science, I feel overburdened with honour. My work has been an absolute pleasure and delight to me. It has never engendered in me a thought of anticipating reward. The crown of science is the joy of its cultivation, and that is nowhere more exquisitely expressed than by Shakespeare when he describes what in his day was certainly the most advanced chemistry—namely, the knowledge associated with the medical faculty. I dare say many of you will remember the quotation I am going to read; it embodies the philosophy that has always appealed to me as a guide in life. Shakespeare is describing, through the mouth of Cerimon in *Pericles*, the qualities of the physician:—

"I held it ever,
Virtue and cunning were endowments greater
Than nobleness and riches; careless heirs
May the two latter darken and expend:
But immortality attends the former,
Making a man a god. 'Tis known, I ever
Have studied physick, through which secret art,
By turning o'er authorities, I have
(Together with my practice) made familiar
To me and to my aid, the blest infusions
That dwell in vegetives, in metals, stones;
And I can speak of the disturbances
That nature works, and of her cures: which gives me

A more content in course of true delight
Than to be thirsty after tottering honour,
Or tie my treasure up in silken bags,
To please the fool and death."

I think that sums up what ought to be the attitude of the truly scientific mind.

With regard to your Grace's observations, may I say that I have now served under three Dukes of Northumberland, not only your father but your grandfather? It was your grandfather who stepped into the breach to help the Institution when it was in grave financial difficulties. It had been in the same position at an earlier period. Before the end of the Napoleonic wars the original design of the founders had collapsed, and the need for a new departure became apparent. This was effected by making pure science the vital basis of its objects, instead of the maintenance of a museum for showing mechanical models of the newest inventions and their application to the problems of life. An Act of Parliament amending the original charter was passed in 1810, and the Institution had to be guided in its new course. This task was undertaken vigorously by Sir Humphry Davy, who had a power of divination that few men have excelled. He saw that in order to stabilise the Institution the aid of women was essential, and his appeal was for an additional number of members, including women, and for the conveyance of a knowledge of science to their children. As no successor has touched the beauty of Davy's oratory, perhaps I may be allowed to read an extract from a lecture he delivered to the members in 1810:—

"Our doors are to be open to all who wish to profit by knowledge: and I may venture to hope that even the female parts of our audiences will not diminish, and that they will honour the plan with an attention which is independent of fashion, or the taste of the moment, and connected with the use, the permanence, and the pleasure of intellectual acquisitions. It is not our intention to invite them to assist in the laboratories, but to partake of that healthy and refined amusement, which results from a perception of the variety, order, and harmony, existing in all the kingdoms of nature; and to encourage the study of those more elegant departments of Science, which at once tend to exalt the understanding, and purify the heart.

"The leisure of the higher female classes is so great, and their influence in society so strong, that it is almost a duty, that they should endeavour to awaken and keep alive, a love of improvement and instruction.

"Let them make it disgraceful for men to be ignorant, and ignorance will vanish; and that part of their empire, founded upon mental improvement, will be strengthened and exalted by time, will be untouched by age, will be immortal in its youth. . . .

"Whatever is to be permanently infixed on the understanding

must be associated with hope or with joy, or with passion. How much more efficacious must instruction be when communicated by an object, beloved and venerated, and in infancy, almost adored; and when, instead of being afforded with an effort of pain and of labour, it is carried into the heart by kindness, and made delightful by caresses and smiles!"

As regards the response made to Davy's appeal, may I say that the proportion of lady members at the present time is practically one quarter?

When I was President of the British Association in 1902, I made a statement, which caused a great deal of comment, about the expenditure of the Royal Institution. During the whole of the nineteenth century the total sum spent on its professorial staff was £54,000. The professors had to live by working outside as well as inside the Institution. The laboratory expenditure was £24,000, and the assistants' salaries amounted to about £21,000. This total of £100,000, with £9,580 contributed by members and friends of the Institution to the fund for exceptional expenditure on experimental research, and £9,600 representing the Civil List Pension of £300 annually paid to Faraday for thirty-two years, really represented the whole of the money cost of the scientific work during one century. What a contribution to the world's progress, and what a meagre amount of pecuniary recognition!

During the lives of Davy and Faraday the Institution received little in the way of money gifts apart from some small legacies amounting to £2,500. During the last thirty years, however, our indefatigable treasurer, Sir James Crichton-Browne, has succeeded in securing benefactions approaching £78,000. Of this sum a large proportion has come from women, and what is interesting, the largest benefaction came from an American citizen who divided his fortune between the Smithsonian Institution, Washington, and the Royal Institution, London. Then there was the splendid benefaction of Dr. Ludwig Mond, who extended our libraries by purchasing the house next door. The congestion we suffered was great, and the fine rooms that were added have been of enormous value.

During the recent war we were naturally in a critical state, and I cannot help referring to what I consider to be one of the noblest anticipations of difficulty and actual benevolence. One afternoon in 1915 Sir Charles Parsons came down to me in the laboratory and said, "You know we are going to have a terrible time, and I am afraid the Institution is going to have a serious arrest. Have you thought it over?" I said that I had. He said, "What do you say we ought to do?" and my reply was, "I anticipate that the membership will drop to one-half in any case; that will certainly be the minimum." He said, "Let me think a moment. Yes, then £5,000 will just meet the loss of entrance and annual fees," and I said it would. He at once sent a cheque for £5,000.

All this shows it is really the ever-helpful members who have been the instrument of maintaining this Institution. It is a family descent, which has become with them an intuition; it passes to the children. The Institution would never have existed, never could have existed, but for the belief that it was not only a noble thing but a proper thing to be able to convey to the young children what science really is by listening to Faraday and the other great professors who have given the Christmas lectures. The Institution has been chiefly maintained from the inside, and the hope is that it will continue to be so. I know of no older club, so called, or combination of people, desirous of following one object and one object alone, learning to tolerate and to hear things they cannot pretend to understand, any more than I pretend to understand anything like the whole of what I have heard to-night. The training that is got here, the learning to sit and try to understand, is something which is really of great value in life.

On behalf of my wife I may say that she has an adoration for the Royal Institution. During the war she suffered from very poor health, and I was very anxious to get her to move to Cambridge, because, as I told her, the Germans would never touch Cambridge. But nothing would induce her to leave the Institution, though had a bomb fallen in our midst the conflagration and the ruins of our library and laboratory, with all its historical apparatus, would have been a terrible disaster in every possible way. We hope to go on for a little longer, but that would be impossible unless we had the support and aid of men of the highest mental calibre and original genius, like the Master of Trinity and Sir Ernest Rutherford, his successor in the chair of Natural Philosophy. If Professor Rutherford adorns the Institution as did Lord Rayleigh and Sir Joseph Thomson, I may pass away in peace, leaving it more secure and more firmly established than when I entered it.

I thank you most gratefully on behalf of my wife. We hope to see you upstairs to look at the cup and to hear my fiddle.

GENERAL MONTHLY MEETING,

Monday, July 4, 1921.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Vice-President, in the Chair.

Miss F. E. I. Smythe.
Mrs. A. J. Webbe.

were elected Members.

The Chairman reported, That, in conformity with the Trust Deed, the Managers had re-appointed Sir James Dewar, M.A. LL.D. D.Sc. F.R.S., Fullerian Professor of Chemistry.

The Chairman announced the decease on June 18 of Sir Thomas Wrightson, Bart., and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss the Institution and the Engineering Industry have sustained by the death of Sir Thomas Wrightson, Bart., J.P. D.L. M.Inst.C.E.

A Member of the Royal Institution for twenty-eight years, Sir Thomas Wrightson served successively on the Boards of Visitors (1918–19) and Managers (1919–1920).

Sir Thomas Wrightson by his business aptitude and spirit of research exercised a beneficent influence on the iron and steel industry, with which he was so closely identified. He contributed many Papers to the Proceedings of various Engineering, Metallurgical and other Scientific Societies. In his Presidential Address to the Cleveland Institution of Engineers he propounded a new theory of hearing, published in a monograph in 1907 on the Impulses of Compound Sound Waves and their Transmission through the Ear. In association with Sir Arthur Keith their investigations resulted in the discovery of many new physiological facts supporting Sir Thomas Wrightson's theory. The whole of the Research is embodied in his work entitled "The Analytical Mechanism of the Internal Ear" (1918). A Friday Evening Discourse on the subject, "The Organ of Hearing from a New Point of View," was delivered by Sir Arthur Keith in 1919.

On behalf of the Members the Managers desire to express their deep sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Geological Survey, Memoirs, Vol. XL. Part 3; Vol. XLIV. Part 1 8vo. 1920-21.
Memoirs: Department of Agriculture, Entomological Series, Vol. VII. No. 3; Botanical Series, Vol. XI. No. 3. 8vo. 1921.
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Astronomer Royal—Report to the Board of Visitors, 1921. 4to.
Astronomical Society, Royal—Monthly Notices, Vol. LXXXI. No. 7. 8vo. 1921.
Memoirs, Vol. LXIII. 4to. 1921.
Batavia, Royal Observatory—Regenval voor 1977 Waarnemingsplaatsen in Nederlandsch Indie, 1879-1917. 8vo. 1917.
Belgium, Royal Academy of Sciences—Bulletin, 1921, Nos. 4-5. 8vo.
Mémoires, in 8vo, Second Series, Tome VI. Fasc. 4-5. 1921.
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British Architects, Royal Institute of—Journal, Third Series, Vol. XXVIII. Nos. 15-16. 4to. 1921.
British Astronomical Association—Journal, Vol. XXXI. No. 8. 8vo. 1921.
British Dental Association—Journal, Vol. XLII. Nos. 12-13. 8vo. 1921.
Buenos Aires, Museo Nacionale—Anales, Tomo XXVII. 8vo. 1915.
Cambridge Observatory—Annual Report, 1920-21. 4to.
Cambridge Philosophical Society—Proceedings, Vol. XX. Part 3. 8vo. 1921.
Chemical Industry, Society of—Journal, June 1921. 8vo.
Chemical Society—Journal and Proceedings, June 1921. 8vo.
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Colonial Institute, Royal—United Empire, Vol. XII. Nos. 6-7. 8vo. 1921.
Edinburgh Society for the Promotion of Trade—Industrial Edinburgh. 8vo. 1921.
Editors—Animals' Defender, July 1921. 8vo.
British Engineers' Journal, June 1921. 4to.
Chemist and Druggist, June 1921. 8vo.
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General Electric Review, June 1921. 8vo.
Journal of Physical Chemistry, April 1921. 8vo.
Junior Mechanics, June 1921. 8vo.
Law Journal, June 1921. 8vo.
Model Engineer, June 1921. 8vo.
Musical Times, June 1921. 8vo.
Nation and Athenæum, June 1921. 4to.
Nature, June 1921. 4to.
Nuovo Cimento, April-June 1921. 8vo.
Science Abstracts, May 1921. 8vo.
Wireless World, June 1921. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LIX. No. 300. 1921. 8vo.
Florence, Biblioteca Nazionale—Bollettino, March-June 1921. 8vo.
Franklin Institute—Journal, Vol. CXCI. No. 6. 8vo. 1921.
Geographical Society, Royal—Journal, Vol. LVII. No. 6; Vol. LVIII. No. 1. 8vo. 1921.

Geological Society of London—Abstracts of Proceedings, Nos. 1073–74. 8vo. 1921.
Geological Literature, 1913. 8vo. 1921.
Groningen, University of—Academia Groningana, 29 Juni–1 Juli 1914 (Tercentenary Celebration). 4to. 1916.
Haarlem, Societe Hollandaise des Sciences—Œuvres Complètes de Christiaan Huygens, Tome XIV. 4to. 1920.
Horological Institute—Horological Journal, June-July 1921. 8vo.
Horticultural Society, Royal—Journal, Vol. XLVI. 8vo. 1921.
Illuminating Engineering Society—Illuminating Engineer, March 1921. 8vo.
Life-Boat Institution, Royal National—The Life-Boat, May 1921. 8vo.
Annual Report, 1920. 8vo. 1921.
London County Council—Gazette, June 1921. 4to.
London Society—Journal, June 1921. 8vo.
London University—Gazette, June 1921. 4to.
Meteorological Office—Daily Readings, April 1921. 4to.
Monaco, Institut Océanographique—Bulletin, Nos. 385–390. 8vo. 1921.
Montpellier, Académie des Sciences—Bulletin, Mai 1919–Dec. 1920. 8vo.
New York Academy of Sciences—Annals, Vol. XXVIII. pp. 167–200; Vol. XXIX. pp. 133–139. 8vo. 1920-21.
Numismatic Society, Royal—Numismatic Chronicle, 1921, Parts 1–2. 8vo.
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Paris, Société Française de Physique—Journal de Physique et le Radium, Tome II. No. 5–6. 8vo. 1921.
Pharmaceutical Society of Great Britain—Journal, June 1921. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLV. No. 7. 8vo. 1921.
Photographic Abstracts, Vol. I. Part 2. 8vo. 1921.
Physical Society—Proceedings, Vol. XXXIII. Part 4. 8vo. 1921.
Princeton University Observatory—Contribution No. 5. Photometric Researches: The Eclipsing Variable U. Cephei. By R. S. Dugan. 4to. 1920.
Rome, Ministry of Public Works—Giornale del Genio Civile, April 1921. 8vo.
Röntgen Society—Journal, Vol. XVI. No. 67, April 1921. 8vo.
Royal Engineers' Institute—Journal, Vol. XXXIV. No. 1. 8vo. 1921.
Royal Society of Arts—Journal, June 1921. 8vo.
Royal Society of London—Proceedings: A, Vol. CXIX. No. 698; B, Vol. XCII. No. 645. 8vo. 1921.
South Africa, Union of—Journal of Agriculture, 1921, No. 6. 8vo.
Department of Agriculture, Bulletin No. 1, 1921. 8vo.
Statistical Society, Royal—Journal, Vol. LXXXIV. Part 3. 8vo. 1921.
Stoll, Sir Oswald (The Author)—"Broadsheets" on National Finance. 8vo. 1921.
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Swiss Chemical Society—Helvetica Chimica Acta, Vol. IV. Fasc. 4. 8vo. 1921.
Tôhoku Imperial University—Science Reports, Vol. X. No. 1; Second Series, Vol. V. No. 3. 8vo. & 4to. 1921.
Technology Reports, Vol. II. No. 1. 8vo. 1921.
Mathematical Journal, Vol. XIX. Nos. 1–2. 8vo. 1921.
Toulouse, Société Archæologique du Midi de la France—Bulletin, Nos. 42–45. 8vo. 1913–19.
United States Bureau of Standards—Scientific Papers, Nos. 399, 401–405. 8vo. 1920.
Circulars, Nos. 32, 99, 106. 8vo. 1920.
Technologic Papers, Nos. 173, 179, 180. 8vo. 1920.
Miscellaneous Publications, Nos. 43, 45. 8vo. 1920–21.

United States Coast and Geodetic Survey—Results of Magnetic Observations at Sitka and Honolulu, 1917–1918. 4to. 1920–21.
United States Department of Agriculture—Experiment Station Record, Vol. XLIV. Nos. 5–6. 8vo. 1921.
Journal of Agricultural Research, Vol. XXI. Nos. 3–5. 8vo. 1921.
United States Naval Observatory—American Ephemeris, 1923. 8vo. 1921.
United States Patent Office—Official Gazette, Vol. CCLXXXVI. No. 2; Vol. CCLXXXVI. No. 2. 8vo. 1921.
Upsala, Meteorological Observatory—Bulletin, Vol. LII. 4to. 1920-21.
Warne & Co., Ltd., Messrs. Frederick (*The Publishers*)—The Koran. By G. Sale. New edition. 8vo. 1921.
Washington, National Academy of Sciences—Proceedings, Vol. VI. No. 12; Vol. VII. Nos. 1–2. 8vo. 1921.
Western Australia, Agent-General—Quarterly Statistical Abstract, Sept. 1920. 8vo.
Yorkshire Archæological Society—Annual Report, 1920. 8vo. 1921.

GENERAL MONTHLY MEETING,

Monday, November 7, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

William Arthur Bond, M.A. M.D. B.Ch. D.P.H.

was elected a Member.

The Chairman announced the decease on July 12 of Professor G. Lippmann; on September 28 of Mr. J. H. Balfour Browne; and on October 28 of the Right Hon. The Earl of Ducie; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted:—

RESOLVED, That the Managers of the Royal Institution desire to place on record in their Minutes their sense of the irreparable loss sustained by the Royal Institution and Science by the death of Professor Gabriel Lippmann, Commander of the Legion of Honour, Doctor of Physical Science, Doctor of Philosophy, Nobel Laureate, past President of the Academy of Sciences, Paris, Professor of Experimental Physics in the Sorbonne, Member of the Bureau des Longitudes, Honorary Member of the Royal Society of London and of the Royal Institution of Great Britain.

Professor Lippmann in 1894 made epoch-making researches in the field of Electro-Capillary Action. He announced to the Academy in 1891 the discovery of photography by means of the interference method and the reproduction of Natural Colours in great variety. The Physical Laboratory of the Sorbonne under his Directorship became the source of many and various original discoveries in Physics.

Professor Lippmann was the author of "Cours de Thermodynamique," "Leçons d'Acoustique et d'Optique," and "Unites Electriques Absolues"; and delivered a Friday Evening Discourse in 1896 on "Colour Photography." He contributed 200 papers on Physical Science.

The Managers desire to express on behalf of the Members their sympathy with Madame Lippmann and the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution desire to place on record their profound sense of the great loss the Institution has sustained by the death of John Hutton Balfour Browne, K.C. D.L. J.P., late Leader of the Parliamentary Bar, Bencher, Middle Temple, Registrar and Secretary of the Railway Commission, Member of many Government Commissions during the War, Author of "Medical Jurisprudence and Insanity," and many works on legal questions, "South Africa," "Essays: Critical and Political," "War Problems," "Forty Years at the Bar," "Recollections: Literary and Political," and numerous contributions to literary periodicals.

Mr. Balfour Browne was a Member of the Royal Institution for thirty-three years. As Vice-President and Manager he always took a keen interest in the welfare of the Royal Institution, and rendered invaluable services by his advice in its management. He delivered two Friday Evening Discourses, the first in

1911 on "Water Supply," and the second on "The Brontës: A Hundred Years After," in 1917.

The Managers desire to express on behalf of the Members their sympathy with Mrs. Balfour Browne and the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to place on record their profound sense of the great loss the Institution has sustained by the death of the Right Honourable The Earl of Ducie, G.C.V.O. P.C. F.R.S., Lord Warden of the Stanneries, President of Clifton College.

The Earl of Ducie was the oldest Member of the Royal Institution. his Membership embracing a period of seventy years. The Earl of Ducie continued his Annual Subscriptions for seventy years, without compounding as a Life Member; and by this means gave his support to the objects of the Royal Institution, which, from its foundation, has been dependent largely upon the aid given by Members of the Peerage. The Earl of Ducie was a Visitor in 1854 and a Manager in 1862.

The Managers desire to express on behalf of the Members their sympathy with his family in their bereavement.

The Chairman declared in the terms of the Bye-Laws. Chapter 4, Article 2, That there was a vacancy in the Office of Manager through the decease of J. H. Balfour-Browne. K.C., and at the next General Meeting on December 5. 1921. the vacancy will be filled in accordance with the said Bye-Law.

The Chairman reported That the President, His Grace the Duke of Northumberland, occupied the Chair at the last Friday Evening Discourse of the Season on June 17. 1921, and thereafter at the request of the Members presented to Sir James and Lady Dewar an antique Silver Cup as a tribute of respect from the Members of the Royal Institution in anticipation of their Golden Wedding on August 8, 1921.

RESOLVED, now that the Golden Wedding has taken place, the announcement be formally reported to the Members at the General Meeting to be held this day.

RESOLVED, That the Speech of the President, His Grace the Duke of Northumberland, on the occasion and the reply of Sir James Dewar be printed for the use of the Members and be subsequently incorporated in a number of the Proceedings.

The Special Thanks of the Members were returned to Mrs. Williams (Granddaughter of Thomas Harrison, F.R.S., Sec. R.I. 1813–24) for her Presentation of Personal Notes of Davy's Lectures at the Royal Institution made by Thomas Harrison in 1808–9. also Classical Works of Cicero, Horace, Hippocrates, etc., and Print of Streatham Park, formerly in his possession.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Research Institute, Pusa: Memoirs: Chemical Series, Vol. V. No. 9, Vol. VI. No. 3. 8vo. 1921.
Bulletin, Nos. 96, 116, 118. 8vo. 1921.
Agricultural Journal, Vol. XVI. Parts 3–5. 8vo. 1921.
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Kodaikanal Observatory Bulletin, No. 47. 4to. 1921.
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Palæontologia Indica: New Series, Vol III. No. 2. fol. 1917.
Survey of India: Professional Papers, No. 18.
Geological Survey, Records, Vol. LI. Part 4, Vol. LII., Vol. LIII. Part 1. 8vo. 1921.

British Museum Trustees—Catalogue of Cretaceous Bryozoa, Vol. III. 8vo. 1921.
Economic Series, No. 12, The Cockroach 8vo. 1921.
Guide to Birds, Part I., 2nd Edition and Plates. 8vo. 1921.
Handbook of Instructions for Collectors, 4th Edition. 8vo. 1921.
Instructions for Collectors, No. 2, Birds and their Eggs. 8vo. 1921.
Handbook of British Lichens. 8vo. 1921.

Accademia dei Lincei, Reale, Roma—Atti, Serie Quinta, Rendiconti: Classe di Scienze Fisiche, Mathematiche e Naturali, Vol. XXX. 1° Sem., Fasc. 11-12, 2° Sem. Fasc. 1-2. 8vo. 1921.
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Rendiconto, Vol. III. 4to. 1921.

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Anglo-Mexican Petroleum Co. Ltd.—Mex Fuel Oil, 2nd Edition. 8vo. 1921.

Antiquaries, Society of—Antiquaries' Journal, Vol. 1. Nos. 2-4. 8vo. 1921.
Archæologia, Vol. LXX. 4to. 1921.
Proceedings, Vol. XXXII. 8vo. 1921.

Appell, Dr. Paul (the Author)—Éléments d'Analyse Mathématique, 4th Edition. 8vo. 1921.

Asiatic Society, Royal—Journal, July-Oct 1921. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. LXXXI. Nos. 8-9. 8vo. 1921.

Bankers, Institute of—Journal, Vol. XLII. Parts 7-8. 8vo. 1921.

Belgium, Royal Academy—Bulletin, 1921, No. 6. 8vo.
Memoires in 8vo., 2e Serie, Tome VI. Fasc. 6. 1921.

Birmingham Natural History Society—Proceedings, Vol. XIV. Part 4. 8vo. 1921.
Geological Work of Charles Lapworth. By W. W. Watts. 8vo. 1921.

Boston Public Library—Bulletin 4.S. Vol. III. Nos. 2-3. 8vo. 1921.

Botanic Society, Royal—Quarterly Summary, July, 1921. 8vo.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXVIII. Nos. 17-20. 4to. 1921.

British Association for the Advancement of Science—Presidential Addresses, 1921. 8vo.

British Astronomical Association—Journal, Vol. XXXI. Nos. 9-10. 8vo. 1921.
Memoirs, Vol. XXIII. Part 4, Vol. XXIV. Part 1. 8vo. 1921.
List of Members, 1921. 8vo.

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GENERAL MONTHLY MEETING.

Monday, December 5, 1921.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

W. A. F. Balfour Browne, M.A.
Dowager Countess of Carnarvon,
Philip Cecil Clifford,
Charles L. Dalziel,
Mrs. Charles Dalziel,
Major Frederick R. de Bertodano,
Lieut.-Col. F. J. Dewes,
Henry Justus Eck, M.A.
Kenneth Lightfoot, O.B.E.
William Maxwell Ogilvie,
Robert Bolton Ransford, M.A. J.P.
Francis Ian Gregory Rawlins,

were elected Members.

Niels Bohr (Copenhagen),
Johan Hjort (Bergen),
Paul Langevin (Paris),

were elected Honorary Members of the Royal Institution.

In accordance with the Bye Laws, Chapter IV. Article 2, Archie Kirkman Loyd, K.C., was elected a Manager to fill the vacancy caused by the death of J. H. Balfour Browne, K.C.

The following Lecture Arrangements Before Easter 1922 were announced :—

J. A. FLEMING, D.Sc. F.R.S. M.R.I., University Prof. of Electrical Engineering, University of London. Six Lectures (Illustrated) adapted to a Juvenile Auditory, on ELECTRIC WAVES AND WIRELESS TELEPHONY: 1. SURFACE WAVES ON LIQUIDS; 2. WAVES IN AIR; 3. THE TELEPHONE: 4. ELECTRIC OSCILLATIONS; 5. ELECTRIC WAVES; 6. WIRELESS TELEPHONY. On Dec. 29, Dec. 31, 1921; Jan. 3, 5, 7, 10, 1922.

F. H. A. MARSHALL, Sc.D. F.R.S., Reader in Agricultural Physiology, University, Cambridge. Two Lectures on PHYSIOLOGY AS APPLIED TO AGRICULTURE. On *Tuesdays*, Jan. 17, 24.

H. H. Turner, D.Sc. F.R.S., Savilian Prof. of Astronomy, University, Oxford. Three Lectures on Variable Stars: Short Period Variables; Long Period Variables; Our Sun. On *Tuesdays*, Jan. 31, Feb. 7, 14.

Sir Arthur Keith, M.D. LL.D. F.R.S. M.R.I., Fullerian Prof. of Physiology, R.I. Five Lectures on Anthropological Problems of the British Empire: Series I. Racial Problems in Asia and Australia. On *Tuesdays*, Feb. 21, 28, March 7, 14, 21.

John W. Evans, D.Sc. F.R.S. M.R.I., Lecturer on Petrology, Imperial College of Science and Technology. Two Lectures on Earth Movements. On *Tuesdays*, March 28, April 4.

Seton Gordon, F.Z.S., Author of "Wanderings of a Naturalist," etc. Two Lectures on Mountain Birds of Scotland; Sea Birds and Seals. On *Thursdays*, Jan. 19, 26.

Sir Napier Shaw, LL.D. D.Sc. F.R.S. M.R.I., Prof. of Meteorology, Royal College of Science. Two Lectures on Droughts and Floods. On *Thursdays*, Feb. 2, 9.

Arthur G. Perkin, F.R.S., Prof. of Colour Chemistry, and Dyeing, University, Leeds. Two Lectures on Dyeing: Ancient and Modern. On *Thursdays*, Feb. 16, 23.

H. Maxwell Lefroy, F.E.S. F.Z.S., Prof. of Entomology, Imperial College of Science and Technology. Two Lectures on The Menace of the Insect Pest; The Balance of Life in Relation to Insect Pest Control. On *Thursdays*, March 2, 9.

P. Chalmers Mitchell, C.B.E. LL.D. D.Sc. F.R.S., Secretary, Zoological Society of London. Two Lectures on The Cimema as a Zoological Method. On *Thursdays*, March 16, 23.

Arthur M. Hind, O.B.E. M.A., Slade Prof. of Fine Arts in the University of Oxford. Two Lectures on Landscape Etchers: New and Old. On *Thursdays*, March 30, April 6.

Charles Macpherson, Mus.Doc. F.R.A.M., Organist of St. Paul's Cathedral, President of the Royal College of Organists, and Prof. of Harmony, Royal Academy of Music. Two Lectures (with Musical Illustrations) on The Evolution of Organ Music. On *Saturdays*, Jan. 21, 28.

Ernest de Selincourt, M.A. D.Litt., Prof. of English Literature, University, Birmingham. Two Lectures on Humorists of the Seventeenth Century: 1. Sir Thomas Browne; 2. Thomas Fuller. On *Saturdays*, Feb. 4, 11.

Ernest A. Gardner, Litt.D., Yates Prof. of Archæology, University College, London. Two Lectures on Masterpieces of Greek Sculpture. On *Saturdays*, Feb. 18, 25.

Sir Ernest Rutherford, LL.D. D.Sc. F.R.S. M.R.I., Prof. of Natural Philosophy, R.I. Six Lectures on Radioactivity. On *Saturdays*, March 4, 11, 18, 25, April 1, 8.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz:—

FROM

The Astronomer Royal—Greenwich Observations, 1916. 4to. 1921.
Heliographic Results, 1916. 4to. 1921.
Catalogue of Double Stars. 4to. 1921.
Variation of Latitude, 1911–18. 4to. 1921.
Astrographic Catalogue, Vol. IV. 4to. 1921.
Cape Observatory Annals, Vol. VIII. Part 5; Vol. X. Parts 5–6. 4to. 1921.
Second Cape Fundamental Catalogue, 1900. 4to. 1920.
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British Architects, Royal Institute of—Journal, Third Series, Vol. XXIX. Nos. 1–2. 4to. 1921.
The Kalendar, 1921–1922. 8vo.
British Astronomical Association—Journal, Vol. XXXII. No. 1. 8vo. 1921.
Observer's Handbook for 1922. 8vo. 1921.
British Dental Association—Journal, Vol. XLII. Nos. 22–23. 8vo. 1921.
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Cambridge Philosophical Society — Proceedings, Vol. XX. Part 4. 8vo. 1921.
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Junior Mechanics, Nov. 1921. 8vo.
Law Journal, Nov. 1921. 8vo.
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Fleming, Professor J. A., M.A. D.Sc. F.R.S. M.R.I. (*The Author*)—Waves and Ripples in Water, Air and Æther. Third Issue revised. 8vo. 1919.
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PROCEEDINGS

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Royal Institution of Great Britain

Vol. XXIII.—Part III. **No. 116**

1922

ALBEMARLE STREET, LONDON, W.1

June 1924

WEEKLY EVENING MEETING,

Friday, January 20, 1922.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D., Manager and Vice-President, in the Chair.

Sir James Dewar, M.A. LL.D. D.Sc. F.R.S. M.R.I., Fullerian Professor of Chemistry.

Soap Films and Molecular Forces.

(The Abstract of this Discourse will be published in a subsequent number of the "Proceedings," together with the Abstract of the Discourse for 1923.)

WEEKLY EVENING MEETING,

Friday, January 27, 1922.

Sir James Crichton-Browne, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Viscount Burnham, C.H.

Journalism.

Writing nearly a hundred years ago the great French writer, Alexis de Tocqueville, said, "The Press constitutes a singular power, so strangely composed of good and evil that liberty could not live without it and public order could hardly be maintained against it." The definition is characteristic in its clean-cut clearness, and does but gain in comprehension by the test of time. Whilst the historian defines he does not explain, and the power of the Press defies anything like adequate and satisfactory explanation. It depends not on scientific data which can be tested and measured and weighed, but on the phenomena of human character which are incapable of scientific analysis. Even to me it remains an insoluble mystery why, because the man in the street sees a name displayed in block letters of

striking colour on a wall or a hoarding, he should walk straightway into a shop and ask for the "proprietary article" in question, which may be recommended by an "ex parte" statement or may be merely the letters of a name. The undoubted fact is, however, that he does so do, and huge fortunes have been made on this simple assumption of human sensitiveness to suggestion. If to the force of the alphabet you can add the excitement of the "flash-light" it seems that by some subtle process akin to the penetration of the X-rays you reach the public mind with even greater success; from which it is quite possible to argue that light has even more influence than colour on the human intelligence. I have been told that Charles Dickens, who carried his mighty genius into every detail of the trade of authorship and had his newspaper experiences to guide him, showed his knowledge of human nature, especially of the common man and of what one of his characters calls the "human boy," in the rules that he observed in the scheme of his advertising. When one of his works was about to appear he took the best spaces available in Central London and covered them with a huge blank space of white: then when the book was about to be published there appeared its title and nothing more. His is the first and the last word in advertising, and the revolving years teach us little more except its variations. We know that coloured paper was deliberately adopted in that belief by certain newspapers. The *Sporting Times*—the "Pink 'Un" of our youth—undoubtedly gained much in prestige and popularity by its colour, although painting the town red dates back to the Bucks of the Regency, if not to the Mohocks of the Restoration. The *Westminster Gazette* gained in the same way by its green colour, which is mentioned in the Life of Sir George Newnes as being something of an inspiration. It was always associated with "the sea-green incorruptible" of Carlyle's "French Revolution," and was supposed for some reason or other to express not only incorruptibility but respectability as well, for Robespierre in his way had both ingredients in his character. It would be interesting to know now whether the present proprietors of the *Westminster Gazette* consider that they have lost or gained by hauling down their colour, for it was distinctive and characteristic, and personally I very much question the expediency of their change over to the common use. The colour was the paper. Why, therefore, should it have been bleached and blanched?

I do not believe that the possibilities of tinted paper, or possibly of colour on paper, have yet been anything like exhausted. In America colour printing is a great feature of the Sunday editions of all the big papers, and it is bound before long here also to lay its hold upon the popular press. I see no reason why headlines and captions should not be printed, as contents bills are now, in different and contrasted colours, or even why colour printing should not be adapted to various kinds of matter for publication: why, for example, politics

should not have a party colour for party speeches or music the corresponding artistic hue.

I pass to another point. In seeking to explain the power of the press the explanation of the eighteenth century still rings true. In his "Thoughts on French Affairs" Edmund Burke says of newspapers that "they are like a battery in which the stroke of one ball produces no great effect, but the amount of continual repetition is effective. Let us only suffer any person to tell us his story morning and evening but for one twelvemonth and he will become our master." There, no doubt, you have the secret of our power. Only three years ago that great sailor, great patriot, and, may I add, great actor, Admiral Lord Fisher, among his many aphorisms, serious and gay, says, "the soul of journalism is repetition." Pounding away, to use Burke's metaphor, or, to vary it, to be able to prepare and to some extent to provide "human nature's daily food," makes the influence of the Press a simpler proposition to all who choose to think it out. To the habitual reader it becomes an unconscious or sub-conscious assimilation of ideas which gradually becomes part of his normal attitude of mind. This is constantly denied. One hears it said repeatedly that the "editorial we" carries much less of its old potency in the public affairs of the country. It may be so, but even if the warning or the exhortation of the leading article has lost some of its force and much of its fervour in these days of self-doubt and self-restraint, there is a make-weight in the other parts of the paper. Mr. Robert Donald said not long since, "Let any who wish write the editorials, so long as I have the news columns to handle." No doubt the relative proportion of actual news collected from all the ends of the earth by news agencies and special correspondents is far larger than it was in the early days of telegraphic transmission, and yet more so, obviously, than when William Howard Russell had to send all his descriptions of the Crimean War by the mail-ship to France and Italy, and thence by a primitive train service. To-day a great newspaper has a service of special correspondents more numerous and more efficient than the diplomatic corps of many a European State. Envoys are sent on special missions to every nerve centre of human interests which for the time being is the ganglion of the fate of nations, and at no period of history has the daily record not only of events but of the great lines of human conduct been anything like so full or so accurate. Excepting in such a "disturbed area" as Soviet Russia or Turkey in Asia, the facilities for the gathering and despatch of news are rapidly reaching everywhere the same standards of mechanical efficiency, and certainly the writers who are doing the work are equal to any of those who have gone before. Sir Valentine Chirol, Mr. Perceval Landon, Colonel Repington, Sir Philip Gibbs and Sir Percival Phillips are wonderful journalists, each in his own style. In addition to the regular hands, newspapers have the call of such pens for special purposes as those of Mr. Rudyard Kipling,

Mr. H. G. Wells and Mr. Arnold Bennett. All these gentlemen write not what are now called "editorials" but articles in the news pages of various journals, so that for that reason only it is small wonder that it is the news features that tend to count for much more in the economy of the press. This tendency, obvious enough at the present day, is in reality no new thing. The great Delane, whether consciously or unconsciously, recognised it, for we are told that after one of his dinners in the best society—and it is recounted of him that one season he accomplished the gargantuan feat of dining out a hundred nights in succession—he went down to his office and sat there till four o'clock of the morning, bringing into accord and consistency the whole of the news and editorial columns of his paper. It would pass the strength and capacity of any man—even of the "Thunderer" of his day—to do it now, that is to say, to co-ordinate, as it would be called, the complete edition to be published *urbi et orbi*.

This brings me to another point. It is said sometimes that no editor ought to touch the news columns of his paper, but there is no doubt that an editor like Delane did deal, and deal drastically, with his news columns. To garble news, as it is called, is quite another matter, but in any wide prevalence of garbling news in the reputable press of this country I altogether disbelieve. News may be, and often is, "tendencious," and the "tendenciousness" may be the result of deliberate deception practised by way of propaganda on a credulous correspondent for political or financial ends. In such an instance the editor is obviously right in altering the copy. Then there is the law of libel, which has a varying importance in different countries and at different times; there is the censorship in time of war here, and it may be at any time abroad by administrative order, with all its pains and penalties either in a court-martial or a court-civil. Upon the shoulders of the editor and his sub-editors falls the full responsibiiity of taking such precautions as will save their enterprise from the huge damages of modern law-suits or the rigours of imprisonment.

The war put the Press under the iron hand of military authority at home as well as on the various fronts, though by the different methods appropriate to British law and custom. Newspaper editors were accorded considerable licence, and after the first few months, when an agreement had been reached between the Prime Minister and the Press at a famous Conference held in Downing Street in February 1915, they were treated on a common basis of patriotic confidence. Cases, however, arose in connection with various papers, and the arm of the law descended upon the editor and publisher. Under the Defence of the Realm Act—commonly called Dora—the Government could either bring those responsible before the magistrate in a police court, or by order of the General Officer Commanding the District could seize the plant and machinery and prevent publication for such time as he had the sole right to

determine, although he was wont to act as a matter of fact on the fiat of the Home Secretary. It so happened in the case of one great and historic journal that a military writer had offended against the regulations, and it was decided by the authorities to take action, but the particular procedure had not been determined when I was asked to go to No. 10 Downing Street for counsel and advice. I strongly recommended the executive officers not to create the scandal of stopping the issue of the paper, but merely to prosecute those who were said to have endangered the military position under the ordinary forms of law which had several times been tested and applied. My advice was taken, and a fine was inflicted by the Court according to precedent. Thus I was enabled in a small way to strengthen the great tradition of freedom of the Press in this country, even under circumstances of the gravest crisis. To-night I merely mention it to show that every day brought to the conductors and managers of the Press heavy risks in credit and circulation.

This toning down or tuning down of war news has nothing to do with the turning and twisting the events of the day to suit a particular line of policy, or still worse a special kind of interest, in any line of life. If it be done, and so far as it is done it merits the condemnation of all honest people, and assuredly in the long run it brings its own Nemesis in the loss of public respect and following, if not in the loss of revenue and circulation.

The selection of news is another aspect of the same problem. The enormous circulations of this century are only possible provided the papers are comparatively easy to handle and relatively cheap to buy, and this of itself is the necessity and justification for what has been called "tabloid" journalism. In Victorian days the great feature of *The Times* was the fullness and accuracy of the Parliamentary reports. It was, of course, the golden age of Parliamentary Government. Lord Chaplin told me that the first speech he made in the House of Commons on Ireland, which he had been studying on the spot during the recess, was reported in *The Times* to the length of five columns. I have no hesitation in saying that such a phenomenon would be impossible now, were the Archangel Gabriel elected as an Independent Member, or even as an Anti-Waster, although his photograph would appear as a main block or as an inset, and not only in pictorial press, for all the press is now illustrated in parts. Immediately the verbatim system becomes either impossible or impracticable, the method of cutting down of or selecting begins, and human nature being what it is, it is exceedingly likely that the reporter, like Dr. Johnson, will not "let the Whig dogs have the best of it." All you can ask for or expect is a reasonable scale of fair dealing, and here again to be violently and vitriolically partisan does not pay in the long run, for it defeats and denies the claim of any newspaper to be really national in character.

In America reporting of speeches or addresses of any kind is very

poor and attenuated compared with our own capacity even in these days. Speeches in Congress are but rarely given, although the members have the right of free printing and circulation in their own congressional areas. This right does not apply to platform speeches. Stenography is much less practised there than here, and the standards of speed and correctness are undoubtedly lower, so that there more than here public men suffer from the misrepresentation that is bound to occur when an oration is compressed into "a stick," to use a compositor's phrase, or when a sentence is wrenched from its context. The American papers have never been so political as ours, and they have continued to greater excess the absorbing interest in crime and criminal stories, which was more marked in our papers a hundred years ago, in proportion to other news, than it is now. Generally speaking, each newspaper in the United States has a whole staff of reporters doing regular rounds of the police stations, or acting as "fixed points" at the central police court, and each furnishes its own detectives to take up and track down every notable crime. Just as our papers had it a hundred years ago, so they still have an intensely personal note in all their interests, political, social and literary, and personal news is even more "featured," to use a journalistic term, in their pages than is the case here even now when we also revel in personal gossip. News stories and editorial comment are all cut and measured to standard. The leading article, the cabled despatch, and the special report are, as a rule, of fixed and universal length, and each variety has its appropriate quota of headlines. There is no branch of American industry which is more machine-made than its newspapers. Their great merit is the smartness of their news service and the quickness of its grasp on the events and interests of the passing show of the world's panorama, as they see it, and of its "clinch" on the public mind of their own section of the great Republic. Because the American Press is given to "stunts" and sensations it must not be imagined that it is devoid of serious features; on the contrary, interleaved with sections for children about Bubble and Squeak, and Mutt and Jeff, which are syndicated over the whole continent, you will find articles on Egyptian archæology or the occult science of Mendelism in cattle breeding written up with a greater display of learning and technology than could be published in any paper of general circulation in this country. The up-to-dateness of the American Press is due to this electricity of its news service. It is not only "an intelligent anticipation of events before they occur," to use Lord Curzon's famous aphorism—although that also is almost American — it is what amounts to instantaneous publication. "Momentariness" has become an ideal of the Press, or shall I say the "stop-press," in every country, and that has come from American initiative. From America, too, has come the prevalent belief in the Press as the natural, nay, more, the inevitable instrument of

democracy in the English sense of the word. The continental character of the United States and its methods of popular government never invested Congress, still less the State Legislatures, with the importance and authority of the British Parliament. Legislative work is mainly in the hands of committees sitting for the most part in private, whilst the executive government works in shadow behind the colossal figure of the President. For most of the purposes of national life it is the newspaper press that embodies and expresses the public opinion, which is always said to rule America more absolutely than it does any other part of the world. If a grievance is to be presented or a scandal to be investigated it is almost always the Press that forces its way into the court-house or the city hall. The exposure of the Tweed ring in the municipal government of New York City and of the shipping orgy of waste in the American dockyards during the Great War are cases in point, one drawn from the last century and the other from the present. As it has been in America, so it is likely to be here, although not quite to the same extent or for the same reasons. It would, however, be a serious loss if no British newspapers were able to publish adequate and elaborate reports of the debates of both Houses of Parliament and of the great public bodies of the country. If it be true that the public life of the country be lived to-day on a lower plane of public estimation, it will only hurry on the process of degradation to exclude trustworthy reports, because it would still further conceal, as in the old days of Parliamentary privilege, what was corrupt and inefficient, and withhold from public knowledge and appreciation all that was sane and sound, in a double sense "of good report" among men.

Akin to the prevalence of "snippets" is the predominance of headlines. In this matter of "captions" we have taken our pattern from the United States, and, as in most of the industrial processes of America, everything in journalism is more or less standardised in the form and quality of the news-sheet. The very length of the paragraphs and the number of the headlines has a curious uniformity, which to my mind is very impressive, for it is not only a powerful stimulant to national solidarity in the "viewpoint," as they call it, of the world's affairs, but a sure proof that in the Union this already exists in fact and in truth. We have no such identity of make-up, but there is a strong movement that way in our cheap press. If the common run of busy people are apt to take their news from the headline, it becomes a shorter cut to intelligence than the paragraph, and there again the matter of selection develops into a fine art of infinite possibilities. "It makes all the difference in the world," said the English philosopher, "whether you put the truth in the first or second place." In newspapers it makes all the difference in the world whether you put the truth in big type or in small, in fat type or in thin, in "caps" or in "ruby." This argument applies for

all it is worth, and, I believe it to be profoundly true, even more to the headlines than to the matter of the printed word. In all these things the newspapers are in a sense one another's keepers, and they correct one another's failings, perhaps not in a month or in a year, but assuredly in the long run of their machine-power over public opinion.

"I don't see anything that railways have done for the world," sneered Ruskin, "except to make it smaller." So it is, of course, with all the machinery of modern life. The interdependence, as it has been termed, of civilisation, which is now being demonstrated so tragically to the whole world, has greatly increased the interest of foreign news. Secret diplomacy, much as might be said for its advantages in the complicated network of human affairs, is not likely to flourish so much or, at any rate, so pretentiously in the future. Public opinion counts for so much more, and diplomacy counts for so little, that the latter must always be seeking the protection and support of the former. During all the many Conferences of various kinds which have followed upon the Great War, a sitting is no sooner over than one or more of the members present, usually the one that is aggrieved by the discussion or by the course of events, rushes to the telephone or pursues his pet journalist or cabinet of journalists to their lair in order to reveal to the full all that has happened, and particularly to glorify the part that he has played in the proceedings. Authorised reports have been made verbally or in writing; they may have been merely "dry bones" and formulas, but the unauthorised reports have left nothing hid from the public view. On the whole this revolution makes for good. It is true it may lead to posturing and playing to the gallery within the council chamber, but this depends more upon the policy of the country than upon the personality of the delegate. When they enter into international Conferences the nations of the world do as a rule what they mean to do and what they have the means to do. Public knowledge is as likely to help the well-meaning as to cripple them, to deter the mischief-makers as to arm them with new weapons of offence. Besides, it was not the case that what was marked confidential was kept confidential. The statesmen of Europe, and especially those of the Continent, have usually had their own organs or even a whole press devoted to their cause. Bismarck was not the only ruler of men in the 19th century who had his "reptile press," although he used and abused it with Prussian thoroughness and Prussian want of scruple. The information that is ladled out and spoon-fed to newspapers in such ways, always partial and mostly misleading, is far more likely to tend to danger and disaster than the most literal account of things as they really happen. I except from this dictum the actual plans and figures of naval and military experts, which so long as war is war, it is necessary to keep in a London fog, even if science generally succeeds in dissipating it by electric wires.

A last point as to the appointment of space in a newspaper concerns the universal love of sport in all English-speaking countries, and to a growing extent elsewhere. It is often said that newspapers devote quite a disproportionate share of their space to sports and games to the damage of serious interests, but if a newspaper is to hold up a true mirror to public opinion, one can hardly say from listening to the man in the street or even the girl in the shop to-day that the reflexion is not true to life. This love of sport has always been a marked feature of British life among the well-to-do, but it was to some extent the indulgence of a class, because the other classes had not the money or leisure to pursue it. Things have changed. All conditions of men and women to-day have a different conception of life and greater opportunities of realising it, so that they take a far greater degree of interest in all the old sports and in many a new one. To respond to this demand the newspapers provide a sporting service of quite a different kind from that which satisfied the 19th century. A daily newspaper in those days had as a rule only one specialist in sport, and he was the racing correspondent; the rest was taken from an agency and written up in the sub-editor's room. Now a big newspaper has a whole battalion of experts dealing with sport in all its branches, and the general reporter hides his diminished head.

Whether the cult of games is not overdone in this country is another matter, but the Press only answers to a universal call on the part of the reading public which will not be denied. To imagine that the ordinary man wishes to live, as Lord Rosebery once put it, "on a blue book and a biscuit" is the pedantry of priggishness, and, generally speaking, the wider and the more various the public taste the better. If a man thinks in terms of football, or if a lad dreams of the skill of Carpentier, he is not so likely to wallow in the Serbonian bog with Lenin and Trotsky. He will take a healthy view of life and the meaning of life, and I prefer the Crystal Palace to the Kremlin.

In all these varieties of the functions of a newspaper you will find recurring one vexed question with regard to which no finality of decision or practice has yet been reached. I refer to the anonymity of the Press. It was, no doubt, the constant rule of earlier days; it was thought to be the golden rule. All sorts of reasons, good and bad, are given to account for its having lasted so long. Historically perhaps the most powerful justification lay in the penalties of the Licensing Act of 1662, and on the force of the panic legislation of 1799, which treated "any house, room, or place, for the purpose of reading books, pamphlets, newspapers, or other publications" as a "disorderly house;" and of the Newspaper Act of 1798, "for preventing the mischief arising from newspapers being printed and published by persons unknown, and for regulating them in other respects." Within five years of starting *The Times* on New

Year's Day, 1785, John Walter the first found himself in Newgate, where he was left in prison for sixteen months. "At eight o'clock," he said, "I am locked up every evening in common with the felons, after which time no soul is permitted to have a person with him. Judge what a man must feel who has till lately enjoyed even the luxuries of life." This sentence he served for the publication of certain paragraphs in criticism of the Princes of the Royal House, which they richly deserved. John Walter was a citizen of high reputation and great respectability. Public men and writers of less degree found it was not easy to escape the pillory, from which he was excused. The myrmidons of Lord Bute and the Duke of Grafton would have given a great deal to discover the author of the "Letters of Junius," and some of the servile judges of those days would have been relied upon to deal faithfully with the scribe. Mr. Fisher, in his excellent book on Napoleonic rule in Germany, relates how the petty German Princes frequently had editors and other pressmen whipped for attacks upon their Governments. If in England it was not quite so bad as this, the proceedings under the law of libel before Fox's Act was passed made the lot of the journalist a hard one, and other punishments might bring with them not less misery and starvation, even if not quite the same amount of degradation. No writer wished to run such a risk could he manage to avoid it, and the publisher and printer were the persons responsible before the law for what appeared in anything they sent out. To this day every newspaper has the name and address of the man who prints and publishes the issue in some part of its contents, whilst in addition, in the case of a private firm as distinct from a limited liability company, the name of the managing proprietor has to be registered at Somerset House for the purposes of the Revenue. For the past hundred and fifty years men of name and fame have been in the habit of writing anonymously in the press, principally on questions of party politics. Disraeli in his early years wrote constantly in the *Morning Post* and *The Times*, and was very proud of his newspaper work, but it was all in the mystery of incognito, which nobody ever loved more than he. Canning, whom he hugely admired, had founded and edited the "Anti-Jacobin" in 1797. It may truly be said that all the great political adventurers, all who did not assume the robe of office by right of birth—or to use Disraeli's own phrase as belonging to the "Venetian Oligarchy"—at some or another in their career wrote leading articles for the daily press. Apart, however, from the legal liabilities of avowed authorship there were also to be considered the conventions of early Victorian respectability. The Press shared with the Stage some at least of the odours of Bohemia, and to acknowledge the connection was not very reputable. When Thackeray, who knew that side of London life surpassingly well, introduced his scenes from newspaper life near the Temple, his newspaper writers bring in their proprietor by the back door, so that he may not run against

their fashionable acquaintances from the West-end of the town. It was said that in *The Times* office it was a matter of etiquette that no leader writer who crossed another in the passage passed the time of day to him, for each was, at any rate by a social fiction, supposed to be ignorant of the other's professional existence. In the middle of the last century *The Times* staff comprised a remarkable body of literary men, yet even to their contemporaries their names were not even shadows, and so their work is not so much forgotten ; it was never identified or acknowledged. This rule applied not only to leader writers but to foreign correspondents, and even to war correspondents. It is true that in regard to the latter effective secrecy was impossible, because their reports were personal, and they had perforce, although not to the same extent as under the Official Secrets Act or the Defence of the Realm Act, to be held individually responsible for them ; but they did not write over their own names. Curiously enough public men, who constantly contributed letters to the editor on public questions, were sometimes better known from *noms-de-plume*, or, as the French say, *noms-de-guerre*, than were the regular members of the staff. Sir William Harcourt was widely known to write on the authority of "Historicus," and the late Mr. Higgins wrote as "Jacob Omnium." Thus, following the early precedents of Steele and Addison, the pseudonym has often been a more famous name than the man's own. The rule of anonymity is by no means abandoned. It continues in the leading article and for the most part in the dramatic and musical criticism, but even in the latter the tendency is to modify it by the signed article sometimes in place of the other, at other times next to it. In the smaller papers, just as is the custom in practically the whole of the American press, the leading articles are reduced to the length of one or two paragraphs, even though they may be multiplied in number.

It is probable, although by no means certain, that the leading article has lost some of its influence as compared with the personal call of the signed work. I should be inclined to say that the relative value is different. In the case of the regular reader the unsigned brings to bear the steady pressure of a consistent policy ; for the occasional reader it is the force of the name that makes the instant effect, fleeting though it may be. It has often been contended, even by those who ought to know better, that for the purpose of identification, or shall I say popular valuation, every article in a newspaper ought to be signed. This does not touch the legal side of the issue, which is sufficiently clear, but the moral side, which is always indeterminate. Henry Labouchere was very insistent upon this topic. He wrote over his own name, except in the days when he was correspondent of the *Daily News* in Paris during the siege of 1870, and he constantly inveighed against the misleading of public opinion by the use of the "editorial we." He judged, however, exclusively from his own example, because, as a matter of fact

and practice, leading articles are not the product of a single pen in any important journal; they are the outcome of consultation and, often, of compromise between the editor and the writer, or it may be the result of an editorial council, in which in these days, especially, the managing partner or the managing director will certainly have a big say. There is nothing new in all this; on the contrary, it is the uninterrupted tradition of British journalism. Leonard Courtney, afterwards Lord Courtney, used to say that he never wrote his leading articles in *The Times*, which he served for many years. "They weren't mine," he said, "they were Delane's." Instructions are given by the editor to the writer, and necessarily so, because the editor has at his command the general sum of information brought from many quarters, which enables him properly to judge the entire issue, whereas the writer has only his personal views to guide him. In such a combination of abilities there is nothing crooked or unfair; on the contrary, it is only thereby that efficiency and correctness can be maintained. Having regard to the numberless channels of inquiry and information and the nature of the co-ordinating and unifying control which I have indicated as the main factors in the conduct of the modern daily journal, you will, I think, realise the force of the remark made the other day to two continents by the late Lord Bryce—that great publicist whom we all lament for the sterling quality of his learned service—that "the greatest of all sources for the present historian are the newspapers." There were certain points, he said, which the historian would have to regard in turning to newspapers. They were:—What are the means of knowledge of the newspaper? what is the responsibility under which it published statements? and whether it does or does not wish to state the truth; and for what class of people does it write? Questions such as these have a Socratic irony about them which make them a little difficult to answer without begging them. When I was a Member of Parliament—I am now on a reduced scale—I heard a story that Mr. Gladstone once said that no matter what the subject under discussion there was sure to rise up a man from the back benches who knew all about it, and this may be even more true of the House in which I sit now. The world of the Press is much the same, except that in a newspaper office these things are settled by prevision and organisation. The necessities of the case require that there shall always be some member of the staff who, if he do not, in the words of the old educational formula, know something of everything and everything of something, has at least special knowledge of some popular branch of knowledge ranging from golf to archæology. What the journalist requires is not merely a bowing acquaintance with art and science, but such a friendly understanding as will give at least a two-power standard of House of Commons expression. Lord Acton is said to have known more and written less than any historian of his time. Every scrap of learning, on the other hand,

that the journalist possesses he is bound by his mere confession to turn to useful purpose, but the lessons that he learns day by day in the moving schools of life so increase its subjective value that every scrap tells its tale. Lord Wolseley, perhaps our ablest soldier since Wellington, publicly confessed that all he had learnt outside the technique of his own profession of arms came from the columns of the daily newspapers. It is true that Lord Salisbury, in his speech on the abolition of the paper duty in 1861, ridiculed the notion that a tax on newspapers was a tax on knowledge. Many things have changed since 1861, but the superior persons are still with us, although I doubt whether even they can afford to dispense with newspaper knowledge in the perplexities of modern times. If they could they would have to be as omniscient as the newspaper is obliged to profess itself. Leaving them on the higher shelf of the human repository, surely it is not too much to claim that the average man gets most of his knowledge, such as it is, from his daily and weekly papers.

The sense of responsibility is not a fixed quantity in any set of people or in any community. It varies with the human factor, and when the sense of duty is low the sense of responsibility will be low also. But like the judges at the time of Queen Victoria's Diamond Jubilee, the newspapers are all conscious of one another's infirmities, and the sense of responsibility is curiously quickened by the vigilance of mutual criticism, for there is no such spur to criticism as competition. It used to be imagined that by constitutional safeguards or by mechanical processes of voting it was possible definitely to fix responsibility upon the holders of power and then to enforce it, but its dispersal among the members of a committee or a corporation on the one hand, and its division by the tricks of the platform and the council chamber on the other, make this a slow and doubtful business. Perhaps Nemesis Dusteropoina in the long run punishes with slow-footed sureness, but she may be so slow as to rob her might of most of its terrors. There never was a time when the personalities of the Press were half so well known as they are now. Lord Northcliffe once said to me on the telephone during the war that he was held responsible for every line that appeared in every one of his papers. Looking at the worry and the hurry of newspaper work I should be prepared to argue that in the eye of the law, and certainly in the eye of the public, the responsibility of the director or the editor or of both is almost too heavy to be borne, and would have been found so even by the unwearied Titans who founded the modern press.

Lord Bryce asks the question whether newspapers mean to tell the truth, and he might best be answered in the words of "jesting Pilate." The truth is the truth as we see it, and it cannot be more than relative to the circumstances of our own time or our own paper. Especially is truth difficult to find in the turbid well of what our American friends call world policies. This I may be allowed to say

without offence, that I believe the standard of veracity to be higher among journalists than in most other categories of men, certainly more than on the political platform or in economic controversies. In the higher altitudes this favourable comparison has applied even to religious questions of orthodoxy and other "doxy." Take an actual example and suppose that at the same time the economic problem of unemployment is being discussed in the public press and on the public platform. The comparison goes deep down to the foundations of national and international life. In the columns of the newspaper the discussion would be carried on in freedom and in truth. Those who addressed the editor can either write over their own names or, as a protection against the popular terrorism of the black list and the branch meeting, over initials or a pseudonym, so that they can speak without fear of reprisals the thing that is in them. Finally, in the leading article which collates the correspondence and draws out its conclusion there would be combined the views of a privy council of experienced journalists, not acting under pressure or with a notice to quit before their eyes. At worst as a shield and buckler, such combatants have professional agreements to ensure their status. On the platform, on the other hand, most speakers are candidates for election or re-election to some public authority, ranging in importance from Parliament to the parish council, or they are office-holders in some trade society or industrial organisation. When they speak they have in their mind's eye a writ of return on the table, or more often, and more forcibly, a writ of ejectment to keep them within the law. "Mass is lord," as George Meredith says, and mass law is a hard law. They are one and all delegates for somebody or another, and as delegates, especially on questions of wages and hours, they are bound by their instructions or their pledges or by both. Their boasted freedom is freedom to speak no doubt, but to speak only according to orders. They are like the representatives of a Government Department in a Legislative Assembly. It is just this power of representing and protecting minorities, and especially small minorities, which is the especial duty and peculiar function of the newspaper press. A great constitutional historian has said that the supreme virtue of our Parliamentary procedure was that it secured and defended the rights of minorities. It does so *pro forma* now, but I doubt if it does so in fact except in the House of Lords, and then only within the limits of our own constitutional impotence. It is only the newspaper press that in tumultuary and storm-ridden days can afford minorities—what Lord Erskine called in 1792 "the last liberty which subjects have been able to wrest from the Crown." Such is the best that can be said for the British Press, the chiefest glory which the best of it can still claim for its colours.

We have behind us a long history dating from the Postboys and the Courants of the early years of the 18th Century. The first daily newspaper was *The Post Boy*, published in London in 1695, and

its existence only lasted four days. The British Press has the longest tradition by which to mould its form and character, but the United States follows not far behind, and Canada in a smaller way has followed the same path. Probably in no country in the world does the newspaper exercise a power so far-reaching and universal as it does in America, but it must always be remembered that relatively to population the size of the reading public here for the daily press is much bigger now than it was in the last century. In this respect there is nothing like the difference there used to be between the two greatest of the English-speaking countries, although, as I have pointed out, our newspapers, coming from a common stock, have branched off on separate lines of development, conditioned in great measure by the divergence of our system of government and national organisation. On both sides of the Atlantic the growth of democracy has thrown the Press into closer touch with the great mass of the people. It is a commonplace to say that the power and prestige of the British Parliament have greatly diminished in this century. In his "Governance of England," Sir Sidney Low carries on Bagehot's disquisition upon our Parliamentary arrangements, and clearly explains how the Cabinet has gained what the House of Commons has lost, whilst the House of Lords has largely ceased to count as a law-making assembly. This process of decay, begun much earlier, has been greatly accelerated by the explosions of the Great War, but public opinion, or at least the opinion of large classes or groups of the people, counts in solid value and effective sanction for as much as, if not more than, it ever did. Public opinion is the sum total of many more voices than it is of minds—*vox populi* is *animus ducendi*—but the predominant minds must have their mode of access, or, as it is so often termed now, their avenue of approach to the mass vote, and to-day it can only have it through the columns of the Press. Before 1885 it was possible to address the electors of a county constituency in the coffee-room of an inn at a farmers' ordinary. Before 1918 it was possible to gather together the electors of a division in a few public halls, or even to canvass them from house to house. With universal suffrage the old electioneering methods have become obsolete, or rather it is not in that way or even at the polling booth that public opinion finds its æolian vent or its compelling force. Especially does this apply to the various minorities, who are howled down at or thrown out of public meetings, and thereafter voted out and, consequently, deprived of representation on the governing bodies of the State. But if there can be no free press except in a democracy, it likewise follows that there can be no democracy without a free press.

Public opinion does not operate only within the four seas or the seven seas, and its influence on foreign policy is likely to be at least as great, if not greater, than on domestic affairs, because internal differences are often subjected to the necessity of showing a united

front abroad. During the reign of Queen Victoria the Press was constantly blamed for all the catastrophes and miscalculations of foreign policy, and the very statesmen who were inspiring the writers of leading articles, and the able editors who controlled them, were the first to throw their blame for everything that went wrong on the Press, even when speaking to the Queen herself. In every volume of Victorian Memoirs there is passage after passage denouncing *The Times* for making it impossible to conduct the foreign policy of the country when first France and then Prussia was being attacked in its columns; yet all the while Ministers were competing for the privilege of inspiring its articles, and Sir William Harcourt, then a young barrister, wrote to Lord Clarendon—himself one of those who was always in contact with *The Times*—that "its exclusive information derived from the Government is nothing less than a letter of credit to the public authorising it to speak on behalf of the Government." So it always has been, and so it always will be. The truth is that the danger of newspaper dictation in foreign affairs arises more often from the Minister's closet than from the editor's room. It so happens that I can supply a good example of what I mean from the admirable volume of Lord Salisbury's Life which has recently been published. In a letter to Lord Bath of December 1877, Lord Salisbury said: "I gather that you write under a firm belief that the D.T., M.P., and P.M.G., represent in some fashion or other the policy of the Government. That the opponents of the Government should say so is only one of what I may call the legitimate injustices of party warfare. But that anyone should seriously think so perplexes me," and he goes on to talk about "impudent pretensions." Well, I happen to know that at that time Lord Rowton, then Mr. Montagu Corry, Lord Beaconsfield's private secretary and *alter ego*, saw my father almost daily. He was the liaison officer between the Prime Minister and the *Daily Telegraph*, so that all the information published came from him, and was in an immediate sense inspired by the head of the Government himself. By this apologia, if it be one, I do not mean that the Press cannot do even more harm than good by adding fuel to the flame of national hatreds or by putting up smoke screens to obscure the real issue and to darken the eternal verities. On the other hand, the same means will avail to soften the asperities of personal bitterness and to cover up the blunders of jealous statesmen and halting diplomatists fencing with one another for tactical advantage. The Press can do more than any other power on earth to promote "sweetness and light" between the nations of the world if it will, and there is no doubt that, as was said by the last Lord Derby but one of England, that from every point of view, moral and material, "its greatest interest is peace." Butler, in "Hudibras," paid a tribute to its deity:—

"Why then let's know it, quoth Apollo,
We'll beat a drum and they'll all follow."

We must hope that the drum will be beaten for the peace march of the world.

Above all it is possible to regard the newspaper press as a power making for fellowship and fraternity within the Empire by explaining to each Dominion of the Crown the wants and aspirations, the ideas and the prejudices of the rest. The "tyranny of silence" would destroy all chance of the British Empire—or as we rather pompously describe it now "the Commonwealth of Nations"—continuing to be a living organism. "Men are not governed as with a charm by dead forms of words," said Burke: but they are a good deal governed by words all the same, and "press notices" have an influence out of all proportion to their permanence in holding together what he called "the mysterious whole." Mr. Hughes, the Prime Minister of Australia, has often said that the interchange of good news and current opinion is the most vitalising force in the concord and concurrence of the Commonwealth. It is remarkable to note how much the newspapers of the Empire have in common. Practically speaking, they have much the same standards, the same principles, and the same ideas. The Australian and New Zealand press has been created in the express image of that of the old country, and except for local variations it has no point of difference from our own. In the Dominion of Canada, American example has counted more than ours, for although the newspapers of Canada and the United States were founded about the same time, the weight of size and numbers has told in the long run, and the process of assimilation has gone along geographical lines. In some things, however, the Canadian press adhere to British practice, especially in refusing to publish a big Sunday edition, and in making Saturday its weekly display. On the whole, there is a great and growing tendency to what Lord Milner calls "moral approximation" in the Empire Press. Since 1910 the Empire Press Union has brought the collective life of the newspapers of the Empire into an organised association, with its central office in London and its autonomous branches in all the Dominions, the Indian Empire, and the Crown Colonies, the main purpose being by harmony and united action to preserve their freedom and independence, to forward their common interests and to extend their legitimate prerogatives. The first Imperial Press Conference was held in London in 1909 under the presidency of my father: the second in Ottawa in 1920 under my own. In a real sense these great Congresses have marked and measured the potency and potentiality of the Press, which may not, and ought not to, eat up the other powers of the State, but which must surely grow equally and equivalently to what economists call the felt wants and the expanding energies of the human race.

[B.]

WEEKLY EVENING MEETING,

Friday, February 3, 1922.

SIR ERNEST MOON, K.C.B. K.C. LL.B., Manager and Vice-President, in the Chair.

LIEUT.-COL. SIR FRANCIS YOUNGHUSBAND, K.C.S.I. K.C.I.E.

The Mount Everest Expedition.

[ABSTRACT.]

THE expedition is being sent to climb Mount Everest because it is the highest mountain in the world, and we consider that there is no part of the earth's surface, and most certainly not the highest point, to which we should not at least try to penetrate.

But are we certain that Mount Everest is the highest mountain? We may be quite certain. The whole length of the Himalaya, both on the Indian and the Tibetan side, has now been explored with sufficient accuracy to ensure that no higher peak exists. And certainly in no other range than the Himalaya is there any peak at all approaching 29,000 feet in altitude.

And the height of Mount Everest we are accustomed to see put at 29,002 feet above sea-level. It is a curiously exact figure, and people are often puzzled to know why that figure 2 is insisted on. The reply is that 29,002 represents the mean of many observations which were made with instruments of extreme accuracy to determine its height. These observations were taken in the year 1849 from six stations in the plains of India, of which the nearest was 108 miles distant from the mountain, and the farthest was 118 miles distant. The lowest altitude computed from these observations was 28,990 feet and the highest was 29,026 feet. The peak was therefore over rather than under 29,000 feet above sea-level.

But a number of corrections have to be applied, and the result of no observations can be taken as absolutely correct. The instrument used for the observations was a theodolite, but no instrument is absolutely perfect. The graduations of the scale may not have been perfectly accurate, and the theodolite may not have been levelled with perfect accuracy. Nor may the observer himself have quite accurately observed. The height of the observing station may not have been accurately determined. The amount of snow on the summit may vary. And most fruitful source of error is the refraction of the

atmosphere. When you are observing a high Himalayan peak from a place in the plains of India more than a hundred miles distant, the ray of light is considerably refracted, and corrections for this have to be made. But what amount of correction has to be applied is not certainly known, and it is now believed that the original computors of the height of Mount Everest applied too great a correction and consequently decreased the height of the mountain. At present the height is computed at 29,141 feet. But further corrections may have still to be made, more particularly on account of the attraction of the Himalaya itself which slightly deflects the levels. And these corrections would add still further to the height of the mountain. So the figure 2 may be taken as very modestly representing the fact that quite certainly the height of Mount Everest is in excess of 29,000 feet.

We know very fairly well the height of the mountain. Why do we want to climb it? What scientific result shall be obtained? The reply to this question is that the Mount Everest Expedition must be regarded as a scientific experiment carried out in the field and on a gigantic scale. It is an experiment to test the capacity of the human race. We know that at those great altitudes the amount of oxygen in the air will be extremely small, but we do not know whether the human organism has the capacity to adapt itself to the unfavourable conditions there prevailing. We cannot know till we try, and the Mount Everest Expedition will try. Not much more than a century ago it was thought remarkable that men should be able to ascend Mont Blanc, which is 14,000 feet lower than Mount Everest. But during the last fifty years higher and higher peaks have been climbed, till an altitude of 24,600 feet has been attained by the Duke of the Abruzzi.

This altitude may be the highest it is possible to attain, but we have not the slightest reason to suppose it is. For experience has shown that as men's minds get accustomed to the idea of reaching higher heights they actually reach them. Once men have climbed to 20,000 feet they think it is possible to climb to 21,000 feet. They make the attempt, and they succeed. Then they want to go on to 22,000 feet, and so on. And perhaps there is the possibility that by extending the *use* of their capacities men actually *increase* their capacities. It seems so when we think of the pantings of the first climbers of Mont Blanc and the matter-of-fact way in which hundreds of mountaineers climb that peak every year nowadays.

But the Mount Everest Expedition will be equipped with oxygen apparatus. The only difficulty in ascending Mount Everest is that presented by the lack of oxygen in the air. The mountain itself presents no insuperable difficulties. By a piece of unusual good fortune it happens to be an easier mountain to climb than any of the next dozen highest peaks. And if climbers had sufficient oxygen they would without serious difficulty reach the summit. But oxygen

has to be carried in cylinders, and the weight of cylinders and the apparatus for holding them is more than thirty pounds for each climber. So the question arises whether the disadvantage of carrying thirty pounds is outweighed or not by the advantage of breathing oxygen. And that can only be tested by experiment in the field.

This, then, is the great experiment which is being made. We are pitting our human resources against the world's greatest mountain, and we mean that our will shall prevail.

[F. Y.]

GENERAL MONTHLY MEETING,

Monday, February 6, 1922.

SIR JAMES CRICHTON-BROWNE, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Mrs. Wilfrid Harris,
Lady Hope.
Miss Adela Shenstone,
Miss Caroline H. White,
John Harold Woodward,

were elected Members.

The Chairman announced the decease on December 11, 1921, of the Right Hon. Lord Halsbury; and on January 2, 1922, of Professor G. L. Ciamician; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

RESOLVED, That the Managers of the Royal Institution desire to place on permanent record in their Minutes, at this their first Meeting since his death, their profound sense of the great loss the Institution has sustained by the death of the Right Hon. the Earl of Halsbury, Privy Councillor, D.C.L. LL.D. F.R.S., late Lord Chancellor, Lord High Steward of the University of Oxford.

Lord Halsbury was an Annual Member of the Royal Institution for a period of sixty years, and thus contributed to the maintenance of the objects of the Royal Institution. During the many occasions he was Vice-President and Manager, his profound legal attainments and his keen personal interest in the welfare of the Institution made him a most revered Counsellor.

The Managers desire to express on behalf of the Members their sympathy with the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution desire to place on record in their Minutes their sense of the great loss the Institution has sustained by the death of Giacomo Luigi Ciamician, Senator of the Kingdom of Italy, Professor of Chemistry, Regia University, Bologna.

Professor Ciamician was elected an Honorary Member in 1899. He attended the celebration of the Centenary of the Royal Institution in that year.

The far-reaching results of Professor Ciamician's researches on the Pyrrol Derivatives, the organic constituents of Plants, and Photochemistry are embodied in a series of more than 130 important Memoirs published in the Transactions of many Scientific Societies. He was the Author of "Chemical Problems of the New Century" (1905); "Organic and Physiological Chemistry" (1908); "Co-operation of the Sciences" (1911); Photochemistry of the Future" (1912); and other works.

The Managers desire to express on behalf of the Members their sincere sympathy with the family in their bereavement.

The following letters from Honorary Members elected at the General Meeting of the Members held on December 5, 1921, were read :—

[Copy]

Universitetets Institut for Teoretisk Fysik,
Blegdamsvej 15, København Ø :
Den 14th of December, 1921.

Dear Sir,

I ask you to kindly bring before the President and Members of the Royal Institution of Great Britain my best thanks for the great honour it has shown me in electing me as an Honorary Member of the Institution. At the same time I beg to acknowledge the receipt of the Official Diploma of this membership.

Yours very sincerely,
N. BOHR.

Colonel E. H. Grove-Hills,
Secretary of the Royal Institution of Great Britain.

[Copy]

Selwyn College Lodge,
Cambridge :
10 *December*, 1921.

To The Secretary of the Royal Institution.
Colonel E. H. Grove-Hills.

Dear Sir,

I beg to acknowledge the receipt of the Diploma signed by His Grace the Duke of Northumberland, and at the same time to express my hearty thanks for the great honour which the Royal Institution through its President and Members has conferred upon me.

Yours sincerely,
JOHAN HJORT.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz :—

FROM

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WEEKLY EVENING MEETING,

Friday, February 10, 1922.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Manager and Vice-President, in the Chair.

W. D. Halliburton, M.D. LL.D. F.R.S.

The Teeth of the Nation.

[Abstract.]

The lecturer began by drawing attention to a series of skulls on the table, which had been kindly lent to him by Sir Arthur Keith. Among these were some of neolithic date which showed perfect dentition; these were compared with the skulls of modern times, which showed in varying degree the ravages of dental caries, or of the equally prevalent disease of the gums and jaws usually known as pyorrhœa. The one modern skull, with a perfect set of thirty-two teeth, he described as the rarest specimen of those exhibited. Caries was not unknown in past ages, and the Rhodesian skull is interesting from this standpoint as exhibiting in its teeth a typical, carious condition. It is, however, undoubted that the ailments in question are much on the increase, and so serious is the problem that a committee under the Ministry of Health is now endeavouring by enquiry and research to obtain the reasons for the spread, and to suggest means for its prevention.

The personal care of one's teeth by cleanliness is the duty of everyone. The mouth is the home of numberless bacteria, some of which produce lactic and similar acids from easily fermentable sugar, such as glucose, which is so much employed in confectionery. If particles of such food are allowed to stagnate around the teeth, the acid locally produced will stagnate also, especially at night, when the normal salivary glow is in abeyance. The acid primarily dissolves the protective layer of enamel, breaches in which allow ready involvement of the dentine beneath.

The tooth-brush is an imperfect instrument, and may, if unwisely used, injure not only enamel, but the gum margin, where pyorrhœa may be started. Immediate attention is then necessary. But even

the best brush cannot get at many possible foci of stagnation, and must be supplemented by the frequent use of antiseptic mouth washes.

Teeth, however, are not merely ornaments to be kept clean and tidy. They are integral portions of the living body, and their successful defiance of outer foes depends on their state of nutrition, and that in turn on the nutritive state of the whole body. Good teeth form one mark of good health, and the principal factor concerned is proper food, not only of the adult, but more especially of the child. Tooth formation begins long before birth, and the healthy feeding of expectant mothers is a necessary prelude to healthy offspring. Reference was here made in general terms to the importance of "vitamins" and the work of Mrs. E. Mellanby on the effect of withholding them on the teeth of puppies.

In reference to diet the lecturer dwelt on the importance of natural foods; many, if not most, of patent and sophisticated foods lack the essential vitamins.

Bad teeth, on the other hand, are produced by intercurrent diseases of many kinds, and the evil is not only a local one confined to the mouth, or even the digestive organs, but they open the way to an invasion of the whole body, and produce numerous ailments (septicæmic, rheumatoid, etc.). Ill-health causes bad teeth and bad teeth cause ill-health, and a vicious circle is established.

A passing reference was made to the marvel of what is called Calcium Metabolism. Various animal cells are able to take up soluble lime salts from the blood, and are able to deposit them as solid structures which permeate the organic matrix of the skeletal tissues, much as corals are capable of building their islands from the lime salts dissolved in the sea. The different kinds of cells employed in bone and tooth formation manifest division of labour, some being able to construct enamel, others dentine or ivory, others bone, and others still of reversing the building process; the last mentioned are exemplified by a certain class of cell which converts the bony material of the fangs of the first set of teeth back again into a soluble state, which operation terminates in the shedding of the milk tooth to make room for its more permanent successor, which also it should be remembered is laid down in the embryo, and so is influenced by the nutritional condition of the embryo and of the mother who harbours it.

The second portion of the lecture consisted in the exhibition of a series of lantern slides (with a running commentary) which illustrated some of the main features in the various structures (enamel, dentine, cement, pulp, nerves, etc.) of the fully formed tooth, and in the development of the original soft dental germ into the fully formed hard structure. Some slides of the closely related tissue, bone, were also shown. In many cases the lecturer took the opportunity when looking at healthy structures to drive home what he had previously said of the

causes of disease and its prevention. Much is being done at the present day, and having no axe of his own to grind, the lecturer was able without bias to urge early attention before it is too late, by visiting responsible dental surgeons for advice and treatment.

But much remains to be discovered in reference to many points, and in his concluding sentence the lecturer ventured to forecast an era in the not distant future when the teeth of the nation would be its pride and not the source of lamentation and pain.

[W. D. H.]

WEEKLY EVENING MEETING,

Friday, February 17, 1922.

Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

D. S. M. Watson, M.Sc., Jodrell Professor of Zoology and Comparative Anatomy, University of London.

History of the Mammalian Ear.

[No Abstract.]

WEEKLY EVENING MEETING,

Friday, February 24, 1922.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

John Joly, D.Sc. F.R.S.

The Age of the Earth.

"The Age of the Earth" is a somewhat ambiguous phrase. From the geological point of view it is generally understood to mean the age of the ocean: in other words, the age of the earth since the beginning of those geological surface changes which are due to denudation. But another meaning may be ascribed to the term. We may assume the beginning to date from the cooling of a highly heated surface to the point of solidification. In this case we include in the age those long periods of Archæan time during which the activity of water played a subordinate part and volcanic commotion prevailed among the semi-fluid, rocky constituents of the globe. Yet a third interpretation refers the birth time to a still more remote and indefinite epoch when the world became differentiated as a planet by activities of the nature of which we are ignorant. Astronomical deductions and speculations regarding the age are mainly concerned with the last period.

What I have to say will be restricted, almost entirely, to the first interpretation of the term. I mean by the age of the earth the period which has elapsed since its surface became the scene of world-wide denudative forces and the foundations of organic evolution were laid.

In virtue of these denudative forces we find ourselves possessed of certain methods of estimating the age which are valid upon the assumption that denudation proceeds in our time at a rate not greatly differing from its mean rate over geological time.

The bases of this assumption are as follows :—

(*a*) That the chief factor in denudative activity being the rain supply falling on the land, solar heat and atmospheric circulation are primary causes. The life on the globe since very early times and the narrow temperature limits conditioning protoplasmic existence and activity show that great extremes of solar radiation cannot have affected denudation for long periods in the past. Mere climatal

extremes do not sensibly affect solvent denudation. Atmospheric circulation, being largely conditioned by the earth's rotation and the distribution of solar heat, cannot have varied to any effective extent.

(*b*) That a considerable percentage of the existing land area being rainless, changes in continental area cannot greatly affect the amount of denudation: the belt undergoing denudation being merely displaced outwards or inwards. The evidence derived from palæography and from the extent of sedimentary deposits in all ages shows that the present land area is not greatly different from the past mean area.

(*c*) That the minor factors affecting solvent and detrital denudation being very many and of very different character are unlikely to combine at any time, and for any long period, in one direction, so as to create a considerable departure from the mean.

Time will not permit a discussion of these statements. I shall refer but briefly to the methods by which the statistics of solvent and detrital denudation are used to afford the age of the ocean.

(1) The chemistry of the ocean and of the rocks is the key to our position. As the result of a comparative study of the primary or igneous rocks and the secondary or sedimentary rocks, we find that, say, n grams of sodium are shed into the ocean for each ton of igneous rock converted into sedimentary rock, and in the ocean we find N grams of sodium. The total denudation over geological time has, therefore, been N/n expressed in tons of denuded igneous rock. Our study also tells us the average total loss attending the conversion of the primary rock into sediment, and so we get the total of the secondary rocks in tons. We now go to the principal rivers of the world, and availing ourselves of estimates which have been made of the amounts of sediment—i.e. of secondary rock material—which they transport from the land in a year, we calculate the number of years it would take to lay down in the ocean the great mass of sediment generated in the past ages. After certain allowances this comes out at about 100 million years.

(2) Again the total of oceanic sodium may give us the age in another and more direct way. We know that by far the greater part of this sodium was carried into it by the rivers during geological time. We turn to the analyses of river water and estimate the total annual supply of this element to the ocean. Dividing the latter into the former and making certain allowances we find an age which is about 100 million years.

(3) A third and more difficult method is independent of our knowledge of chemical denudation. We estimate the maximum thickness of the integral sedimentary deposits, and knowing the burthen of sediment conveyed per annum by the rivers, we estimate the maximum thickness of deposit annually derived from the same; we divide the latter into the former and find an age which, again, is about 100 million years.

Of these methods, that which involves the sodium modulus only is the most direct. Of course the reason for selecting this particular element as a modulus is because of its great solubility, on account of which it alone among the dissolved oceanic constituents has been preserved from organic abstraction or chemical precipitation. This method has been examined by many critics. Notably by Sollas, who, in a presidential address to the Geological Society in 1909, subjects it to searching examination. He concludes that a period of 175 millions of years may be reached upon certain assumptions, and that this must be very nearly the maximum allowable. My own examination of this method has led me to believe that it is *possible* that 150 millions of years may be indicated by it, and that 200 millions of years would not be reconcilable with our present knowledge of the factors involved. This would, as I have already stated, apply only to the duration of sedimentation. It cannot be compared with data which apply to an age dating back into the Archæan.

There was, indeed, some scanty sedimentation in Archæan times. We cannot form any estimate of its effects upon our numerator or upon our denominator save that we seem entitled to conclude that they were small. "The Archæan was essentially a period of world-wide vulcanism, and in the relative proportion of rocks of igneous and sedimentary origin represents a departure from the uniformity of conditions of later geological time." I quote from the monograph of Vane Hise and Leith.

Before passing on to the results based upon radio-activity I must refer to one point in particular which has been urged against accepting present-day rates of denudation as a basis of time measurement. It is said we live in a period of abnormal continental elevation which, it is asserted, involves excessive solvent denudation. A little attention to the nature and conditions of solvent denudation should have sufficed to forestall the argument. But a ready method of dealing with it is available. The continent of North America has a mean elevation of 700 metres: it is being denuded at the rate of 79 tons per square mile per annum; for South America the corresponding figures are 650 metres and 50 tons. Now Europe has a much lower mean elevation—300 metres. Its rate of denudation is, however, 100 tons per square mile per annum. The rate of solvent denudation is, in fact, by measurement found to be *less* for the more elevated land, as theoretically it should be. The argument then, if it has any basis, would indicate that the age as found from solvent denudation is excessive.

Prior to the advent of those methods for investigating the earth's age, which are based on radio-active changes in the elements, no serious objections to the results reached by the geological methods were raised, so far as I know. There were some, indeed, who regarded the age as excessive. Thus Becker arrived at a lesser figure by taking into account the progressive impoverishment of the

surface materials during geological time. The validity of the correction is, however, open to doubt. Others considered that the organic changes recorded in the rocks required a longer period. Sollas gave, as I think, a clear answer to this objection in his "Age of the Earth." Both Lyell and Geikie, and Poulton, had in past years upheld the doctrine of Uniformity. But the advent of the radioactive method, as founded on the uranium family of elements, seemed to point to a vastly greater age; leading, in fact, to the extraordinary conclusion that the present rate of solvent denudation is not less than four times, and may be eight (or even more) times, in excess of the average rate obtaining during the past.

The earliest suggestion of the possibility of using the stored-up products of radio-active change came from Rutherford. He, and later Strutt (now Lord Rayleigh), applied the accumulation of helium to the evaluation of geological time. Strutt laid out a geological chronology, the first of its kind, but considered he was dealing with minor limits. Boltwood used the residual product of uranium—lead—and for Archæan (?) materials reached as much as 1640 million years. As I have already said, the denudative method cannot be regarded as extending to those remote times. But such results as 480×10^6 years for Silurian or Ordovician deposits, and 1200×10^6 years for Post-Jatulian, are quite out of harmony with the denudative method. To-day the matter stands thus:—A number of results are available based upon the use of carefully selected material, and when the material is thus selected the ratio of lead to uranium—the "lead ratio" as it is termed—increases as we go downwards and diminishes as we go upwards in the strata, preserving a fair degree of agreement even for widely separated localities.

Those who would rest content with this result, however, can do so only by ignoring the very interesting and suggestive fact that when we base the results on the lead ratio of selected thorium minerals, we arrive at ages which are in substantial agreement with the results reached by the denudative method. On the face of it this agreement gives strong support to the conclusions reached by methods absolutely different in nature.

For long it was known that thorium minerals—such as thorite—gave persistently lower ages than uranium minerals. It became the custom with some to treat these ages as untrustworthy. But we know now that this attitude is not justified, but rather that the onus of explaining away the impressive agreement between the indications of thorium lead and denudative statistics rests with those who would reject the age supported by both.

Soddy's determination of the atomic weight of the thorium lead isotope in 1917 afforded material for an age determination on a very large scale, and from the nature of the research, one of special value. The material was a thorite from Ceylon; from rocks immediately overlying the Charnockite series. The latter is extremely

ancient—Lewisian or Lower Archæan. Upon reading in "Nature" Prof. Soddy's account of his determination of the atomic weight of the lead derived from these rocks, I estimated that the quantity of lead extracted from the thorite gave an age of 130 millions of years for the time since this mineral had been generated; and on communicating with Prof. Soddy I found that he had reached a somewhat similar conclusion.

At this time, however, there was the possibility that thorium lead was not altogether stable. Suspicion fell more especially on thallium as the final product. Two experimental results, however, laid this doubt to rest; experiments upon a thorianite made in my laboratory by J. R. Cotter failed to detect even spectroscopic traces of this element, and there was insufficient thallium found in the thorite dealt with by Prof. Soddy. In a subsequent letter to "Nature" Prof. Soddy states that a research carried out at the Radium Institute of Vienna supported the view that the lead isotopes derived from thorium were both stable. I shall refer presently to yet additional evidence that the transformations of the thorium family cease with lead.

Writing to "Nature" in support of the hypothesis then under discussion—i.e. that thorium lead was unstable—A. Holmes cited a result on a selected specimen of uraninite, showing that the rocks in which Soddy's thorite occurred were, according to the uranium-lead ratio, 512 millions of years old. Previous uranium-lead ratios had assigned a much greater age to them. Here, then, the results join issue: the uranium result is just four times as great as the thorium. We notice, too, that on the uranium-scale of time this thorite must be older than Silurian or Ordovician, which have been determined by uranium lead as 430 millions of years ago. Probably its age dates back to Cambrian or even to pre-Cambrian time. From what we have already inferred we cannot regard 130 millions of years for early Palæozoic time as irreconcilable with the maxima which denudative methods afford. More recently, lead derived from a Norwegian thorite of Langesundfiord—also of lower Palæozoic age—seems to reveal an age of 150 millions of years. In this case, also, there is the added security of a determination of the atomic weight of the lead.

We cannot discredit these results on the score of radio-active instability of the lead. Why, then, set them aside in favour of results reached on uranium lead, which are in hopeless contradiction to the indications of the record of the surface activities of the globe? It is, indeed, not too much to say that the whole position is now reversed, and that to-day suspicion attaches to the uranium-lead ratio. And, as we shall see, there is much unknown about the earlier radio-active sequence in the uranium series, while the discovery of isotopes opens the way to possibilities unthought of in the earlier days of radio-active science.

I shall, however, now turn to the evidence of the pleochroic halo on this matter.

The halo affords a means of investigating certain facts respecting the break-up of the radio-active elements in the remote past. For the dimensions of the halo—minute though they be—can be determined with considerable accuracy, and these dimensions are conditioned by the added effects of the several α-rays emitted by the transmuting elements. Bragg and Kleeman observed and measured just such integral ionisation effects in air. In the rocks the ionisation curves, owing to the great stopping power of minerals, are on a scale 2000 times as small. They are very faithful hieroglyphics, however, and carry back our knowledge over an appalling vista of time.

One single α-ray produces a well-known curve of ionisation determined by Geiger. The range of the rays does not affect the general nature of the curve. If we imagine uranium or thorium as parent elements contained in a minute crystal—of zircon, for instance—we must picture the various α-rays affecting the surrounding substance—mica, we may suppose—in such a way as to build up concentric spherical shells more or less overlapping and corresponding to the radial distances at which the ionisation of the several rays is at a maximum. As seen in section upon cleaved flakes of the mica, we find concentric coloured rings representing the ionisation due to the rays.

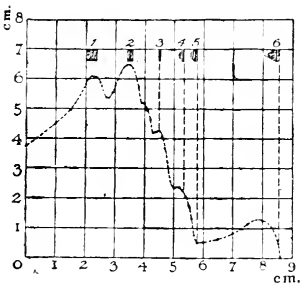

FIG. 1.

In order to arrive at the theoretical location of these rings we must add up the several ionisation effects as observed in air. This involves assigning a Geiger curve to each ray according to its range and adding up the ordinates.

Let us consider first the case of the thorium halo. Fig. 1 is a curve arrived at in the manner I have just described. Its ordinates are proportional to the integral ionisation effects of those radio-active elements in the thorium series which emit α-rays. And above it I have marked, calculated into the range in air, the positions of the coloured rings which in biotite we observe encircling a minute mineral particle containing thorium and all the successive products of its transmutation. This, of course, necessitates magnifying the halo enormously—rather more than 2000 diameters. You perceive that the halo very faithfully conforms to the features of the air-curve. It may be of interest to mention that the finding of the third ring led to the discovery of the prominence on the curve which accounts for it. This part of the curve had originally been plotted from an

insufficient number of ordinates. This close agreement really reveals a very important fact. The air-curve depends for its dimensions on the ranges of the several α-rays as we measure them to-day in the laboratory. The halo-measurements refer to radio-active effects which began their record in this mica in Carboniferous times—possibly long before. The halo reveals no sign of change in the several ranges concerned. As you are aware, the rate of break up, the transformation constant of the element, is related to the range. We are, therefore, in the case of the thorium family, entitled to read in these minute and ancient records a guarantee that the accumulation of the final product—the thorium isotopes of lead—was in the remote past effected at just such a rate as we have inferred from the splendid researches of our day. The thorium halo gives us this guarantee. It also tells us that it is improbable that the resulting lead is unstable. For if it were we must find room for rays additional to those we have used in deriving the ionisation curve. True, a coincidence of range might enable a ray to lie concealed in the halo; but the fit of the halo is so absolutely faithful to every feature of the curve that this seems improbable.

It is also possible to observe the successive stages of development in thorium haloes. The first rings to appear are those corresponding to the two conspicuous crests of the curve, Fig. 1. If the central nucleus is small or feeble, nothing more may be developed.

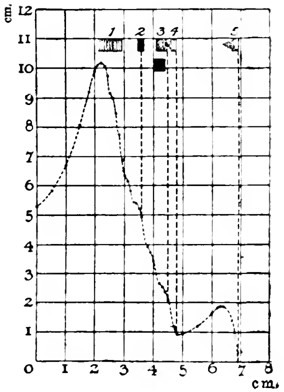

Fig. 2.

We now turn to the uranium curve. The eight contributory ionisation curves are placed according to the range of each ray, and Fig. 2 shows the curve produced by adding up the ordinates. Above it are laid out the several rings observed in the uranium halo. Looking at these rings, we notice that the outer features of the halo seem in fair agreement with the present-day ranges. But the innermost ring has a larger radius than would be expected from the curve. Much care has been expended in verifying this point. In the Devonian mica of County Carlow these haloes are found in every stage of development according to the size or activity of the nucleus. The uranium halo begins as a single delicate ring surrounding the minute central nucleus. It can be measured from a stage bordering on invisibility to a stage when its central area is beginning to darken up and the first shadowy signs of the outermost ring of all—that due solely to radium C—appear. A large number of readings on these embryonic haloes, made recently by various observers, confirm the

mean value of its radius as cited in a paper communicated to the Royal Society in 1916. The discrepancy with the theoretic curve is small : 10 or 12 per cent. of the external radius. The allowance for, and measurement of, the nucleus is sufficiently difficult to introduce some uncertainty.

This misfit may be of considerable significance. I have already reminded you that the range of the α-ray emitted by a transforming element is related to its rate of break-up. The range is longer for the shorter-lived elements. Now, here the first ring of the uranium halo in mica shows a longer range than we would expect from the air-curve as observed to-day. The agreement between the two in other cases appears to show that this is not due to any unknown effect influencing the retardation in mica. The location of the first uranium ring is mainly referable to those short-range α-rays arising from the initial transformations of the uranium series. We infer that one or more of these rays must have had a longer range in past times, and, of course, that the corresponding transformation periods must have been shorter. A specially influential ray is that slowest of all the rays—that which is emitted in the break-up of uranium 1. The discrepancy might be due to this ray possessing a greater range in early geological times. But, whatever the cause, the nature of the misfit suggests evidently that formerly the rate of transformation of uranium to lead was faster than it is to-day.

It is with some reserve that I refer here to measurements made lately on haloes of comparatively recent and of very remote geological ages. I say "with reserve," for not only are the results of a nature calling for very adequate confirmation, but the measurements present considerable difficulty. The point at issue may be stated in a few words :—Is the abnormality observed in the dimensions of the uranium halo dependent in amount upon the antiquity of the rock in which the halo is developed ?

I had sought occasionally for uranium haloes in rocks younger than the Leinster granite—which is of early Devonian age. The granite of Mourne, which is of Eocene or early Tertiary age, for long refused to reveal any haloes suitable for measurement. However, recently I was so fortunate as to find a few of these early halo rings which I was able to measure. Further search has revealed a few more ; but they are excessively scarce and rather difficult to detect. The nuclei of these haloes are only rarely zircon—they seem to be apatite ; possibly allanite—and their average size is greater than the zircon nuclei of the Carlow mica. Both the mineral nature of the Mourne nuclei and their dimensions involve, therefore, a bigger subtractive correction on the observed radius than is required in the case of the Carlow haloes. But in addition to this, there appears to be a small difference in the external radius of the Eocene halo and that of the Devonian halo. According to a large number of readings by several observers, some of whom were not acquainted

with the question at issue, the external radius of the Eocene halo-ring—no allowance being made for the nuclear radius—is 0·0135 mm. The same observers obtained for the Devonian halo 0·0146 mm.—without allowance for the nucleus. The nuclear correction, as I have said, would have increased the discrepancy, but the correction is a difficult one. There is no reason to believe that *more* than 1 per cent. of this difference can be ascribed to the chemical composition or density of the micas, both of which have been investigated.

Still more recently I have found these primary ring-haloes in the micas of Arendal and Yitterby, which are said to be of Archæan age, and which are certainly extremely ancient. These haloes appear to possess a radial dimension of 0·0160 mm., or a little less. Here, again, the nature of the mica does not appear to be responsible. According to these measurements it would appear that the radius of the Eocene halo-ring must be increased by about 7 per cent. to attain the size of the Devonian halo-ring, and that this is, in radial dimension, about 10 per cent. smaller than the Archæan. It would seem as if we might determine a geological chronology on the dimensions of these halo-rings!

The foregoing results, if confirmed, would give strong support to the view that some factor, variable over geological time, had affected the ranges and periods of certain elements concerned in building up the uranium halo. However, too much stress must not be placed on these measurements till they are confirmed by haloes in yet other micas. Pending further investigations, I return to the fact that the uranium halo of Devonian age does not conform to the ionisation curve of the uranium family as determined on present-day measurements. Serious discrepancy seems confined to the shorter ranges, more especially with that primary range which is most influential in determining the rate of production of uranium lead.

We do not appear to be in a position to deny the possibility that uranium 1 may have slowed down in its rate of decay over geological time. Such laboratory observations as can be extended to the case of short-lived elements would not, probably, shed any light on the matter. It is a possibility long ago suggested by Rutherford. But if this is the explanation we must admit that in the case of thorium any corresponding effect must have been much smaller. On the whole the former influence of one or more isotopes of uranium—which possibly may almost have disappeared—seems the more probable explanation. Hypothetical isotopes of uranium have been invoked by highly competent authorities to meet the difficulties affecting the ionisation accounts of the uranium family of elements. Boltwood suggests as "not impossible" that what we now call uranium consists of three radio-elements: a parent element and two isotopic products all emitting α-rays.* In 1917 A. Piccard put

* Phil. Mag., 6 S. vol. xl. p. 50, 1920.

forward the view that the parent of actinium is a third isotope of uranium not belonging to the uranium family and having an atomic weight of 240. This view is regarded favourably by Soddy and Cranston. It clears up the difficulty respecting the atomic weight of uranium, and fits in with the atomic weights of radium and of uranium lead. Soddy and Cranston remark that in order to explain, in this case, the constant ratio of actinium to uranium observed in minerals we must suppose the period of uranium 1 and of the hypothetical isotope to be the same. This difficulty, however, is removed if we may assume that the ratio varied over geological time.

A somewhat similar theory to Piccard's may be invoked to explain the abnormality of the Devonian uranium halo. We have these facts to go on:—The age indicated by uranium for Lower or Pre-Palæozoic rocks is about four times too great as compared with the age indicated by thorium. We assume, therefore, that three-fourths of the lead as measured in uranium minerals is derived from a certain isotope. This isotope, not having been detected in our time by its primary α-radiation, we must suppose to be now sensibly exhausted. We, therefore, have a known mass of this isotope transforming to lead in a known time—130×10^6 years. Assuming that only 1 per cent. of it is left we get its transformation constant ($3 \cdot 5 \times 10^{-8}$), and by Geiger and Nutall's relation we find the corresponding range as $2 \cdot 6$ cms. at $0°$ C.; or about $2 \cdot 75$ cms. at $15°$ C. To-day the α-radiation of the hypothetical body would be only $\frac{1}{1000}$ of that due to uranium 1, but during the period since the Devonian there will be about three α-rays from the short-lived isotope to one from the long-lived. The integral curve of ionisation as modified by these hypothetical results would be in agreement with the Devonian halo. We have to assume that the ranges of the rays emitted by the successive disintegrating products of the supposed isotope were such as to leave the outer features of the halo sensibly undisturbed. This seems not improbable.

The salient features which appear in the study of radio-active haloes are:—Firstly, that the agreement of our laboratory measurements of to-day with the features of the Palæozoic thorium halo is such as to support the view that the periods of the several elements concerned in its genesis have remained unchanged over 130 millions of years. This fact, taken along with the stability of thorium lead, seems to render its reading of geological time authentic in a high degree. Its indications are confirmed by the consistent testimony of the denudative processes which have progressed on the earth's surface. Secondly, it appears that the uranium halo is not in conformity with the period we ascribe to-day to uranium—a disagreement which is emphasised by the failure of uranium-time to conform with the united testimony of thorium-time and denudative-time; as well as by much that remains unexplained respecting the earlier changes in the uranium family of elements.

The complete tale is not yet told, but I think the balance of probability is in favour of an age between 150 and 200 millions of years for the earliest advent of geological conditions upon the globe.

Astronomical investigation on the subject of the age of the earth deals, generally, with that greater age which must be ascribed to the earth as a planet. For this age vast periods have been claimed. But it is possible to reconcile superior ages for the earth as a planet with comparatively brief geological time. And, to my mind, in doing so we proceed upon what is no more than a necessary deduction based on our knowledge of the radio-activity of terrestrial materials. I would go further—still, as I believe, logically—and ascribe to radio-active energy an influence on planetary and stellar evolution much greater than has hitherto been admitted.

The only planet we can investigate at all closely is, of course, our earth. And what do we find? In its surface materials there are sufficient of the radio-active elements, as Lord Rayleigh first showed, to account for the observed average temperature gradient if the surface conditions extend a little way, about 19 kilometres, inwards. It is, for many reasons, in the highest degree improbable that such a definitely defined radio-active layer exists. Nor is it probable that the earth's interior is free from radio-active substances. We find both uranium and thorium in meteorites containing a large percentage of iron and nickel, and, although they have not as yet been found in meteoric iron, we know from the mean density of the earth that its interior cannot be composed of pure iron. It is probable that a considerable proportion (some 40 per cent.) of siliceous materials are intermingled; and when such exists in meteorites invariably we find the radio-active elements. By what conceivable activity was all the uranium and thorium separated out and brought to the surface?

The view that radio-active elements exist in the earth's interior is sometimes met by a formal denial that the earth can be getting hotter within. Upon what evidence is this denial based? If the central core of the earth for a radial distance of 2000 kilometres, say, had risen in temperature by 1000° C. over geological time—and upon a low assumption of the interior radio-activity it might reach this temperature in 150 million years—would we be aware of the fact? Would the day be appreciably lengthened? Would there be any effect at all if the outer parts were cooling due to loss of primal heat? We have further to consider that only over the short period of historical time would any observations be available. The denial is quite baseless so far as my estimates go.

Well, then, if our earth is heating up within, is there not an impending termination to our geological age? Kelvin showed how complete is the thermal isolation of the earth's interior, and it is certain that interior heat is not now escaping. The rise of tempera-

ture within must go on till the present epoch succumbs to the accumulated energy. Then must ensue a period of vulcanicity which will end life upon the globe, and probably reverse the chemical work stored up by ages of denudative and organic activity. The whole sequence of events — rapid cooling by radiation, restoration of the oceans, and, possibly, re-birth of life and of its evolutionary history—would begin all over again. On this view the age we have been studying may be one of many, and will inevitably attain its three score and ten, terminating in labour and sorrow. But there must come a rejuvenation, and the rejuvenation, possibly, may one day be pondered by other minds than ours. Remember that after some ten thousand millions of years there still survives 50 per cent. of the heat-generating elements, and the effect of their diminution is only to lengthen out the recurring geological ages. Our planetary companions may be in various stages of such cyclical changes.

[J. J.]

WEEKLY EVENING MEETING,

Friday, March 3, 1922.

Sir James Crichton-Browne, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

C. Morley Wenyon, C.M.G. C.B.E. M.B.,
Wellcome Bureau of Scientific Research.

Microscopic Parasites and their Carriers.

Microscopic parasites, like the living things which we see around us, belong either to the vegetable or animal kingdom, and I propose to deal with the latter group alone, and more especially with reference to the various methods by which they are handed on from one host to another, "host" being the term applied to the larger animal or man in which they live as guests, however unwelcome they may be. I wish particularly to draw attention to the relationship existing between the host and its parasite, and to show how a parasite may avail itself—if one may suppose for convenience that so lowly an organism can deliberately avail itself of anything—of the habits and peculiarities of a blood-sucking invertebrate for the purpose of transferring itself to a second host in order to keep itself in being.

The one essential characteristic of all living things is the capacity they possess of propagation and maintenance of the species. This characteristic is necessary for their continued existence, and should it fail the species will at once become extinct. The various methods by which parasites in general maintain themselves—methods which are sometimes extremely complicated and necessitate the passage from one kind of host to another—form a most fascinating subject for study which not only is of interest in itself, but which has a direct bearing on the welfare of mankind in the prevention of disease.

It is a general notion that a parasite is distinctly harmful to its host, and in a wide sense this conception is true But it is easy to realise that if a parasite gains entrance to a host and multiplies so rapidly that the host is quickly killed, the chances of the parasite finding a home in another host may be very small indeed. This danger of extinction is a very real one, for the parasite has a free-living ancestry which led the usual competitive existence amongst innumerable creatures around it. In adopting a parasitic mode of existence it has lost most of the qualities which enabled it to hold its own in the world of living things. When it has adapted itself to

the natural protective powers of its host, it lives a secluded existence and runs little risk of being exterminated by other organisms. It becomes structurally modified, and more and more unsuited to life in the world around it. It is no longer able to survive outside the body of its host, and in order to bring about its transference to new hosts one of two things happens. Special resistant forms are produced, if there is a possibility of external exposure on the ground or in water; or special stages are evolved which can continue their development in hosts, such as blood-sucking insects, which are capable of taking them up directly from the blood. The production of these special forms takes time, and if they are not produced before the host is overrun and killed, all the parasites in that host will perish with it. Accordingly, it is found in nature that a definite balance is struck between the host and its parasite. The latter does as little damage as possible so long as it can multiply satisfactorily and finally produce the transmission forms, while the recuperative powers of the host are continuously repairing the damage done.

By the time the special transmission forms of a parasite have appeared, and there has been a sufficient interval for their transference to another host, the parasite may cease to multiply, those already present die off, and the infection of the host automatically comes to an end. On the other hand, the damage done, though not killing the host rapidly, may do so gradually, the parasite being not inconvenienced by this unpleasant result so long as it has attained its object in enabling its offspring to gain a footing elsewhere.

It can be accepted generally that instances of infection with parasites which bring about the death of the host in a short time are unnatural ones. The trypanosomes, which cause nagana diseases of cattle and other domestic animals in Africa, produce a virulent and rapidly fatal infection, but these animals cannot be regarded as the natural hosts. The latter are to be sought for amongst the wild game which harbour the same trypanosomes without showing, as far as can be detected, any symptoms due to their presence. It is this fact which makes it extremely difficult to exterminate the disease in Africa, for the tse-tse fly has every opportunity of infecting itself from the game and handing on the trypanosome to unnatural hosts, such as the domestic animals. In the case of the parasites of malaria we have another illustration of the same fact. A native who has been infected with the parasites from his childhood upwards, though still harbouring them, suffers comparatively little; but immediately unaccustomed hosts, in the shape of Europeans, become exposed to the same infection, as in the case of the British armies in Macedonia, East Africa and elsewhere, the story is a very different one.

Under natural conditions there is a definite balance between host and parasite which holds good for all classes of infection, with few exceptions. If we seek the reasons for this balance we are led into the questions of immunity and tolerance and virulence of particular

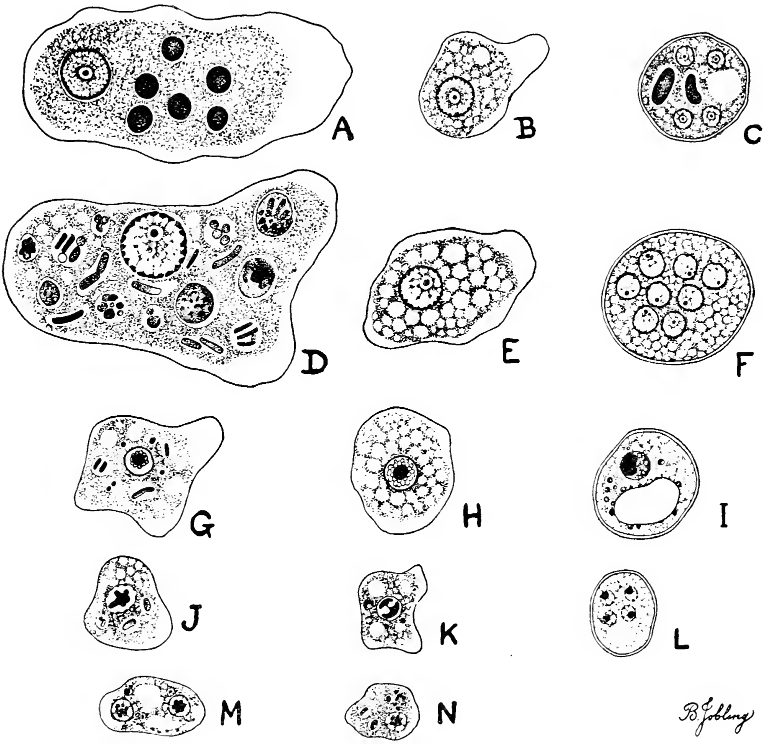

FIG. 1.—THE INTESTINAL AMŒBÆ OF MAN, WHICH ARE CARRIED FROM MAN TO MAN BY CYSTS VOIDED IN THE FÆCES. (MAGNIFIED 1250 TIMES.)

A–C *Entamœba histolytica*, the cause of amœbic dysentery.

A, the tissue-invading form with one nucleus and six ingested red blood corpuscles; B, the small form (precystic form), which will protect itself by a cyst; C, the cyst containing the amœba, with four nuclei and chromatoid bodies.

D–F *Entamœba coli*, the large non-pathogenic amœba of man, which lives in the lumen and on the surface of the intestine.

D, the large multiplying form with one nucleus and various ingested food bodies; E, the small precystic form, which will become encysted; F, the cyst containing the amœba with eight nuclei.

G–I *Iodamœba bütschlii*, another harmless amœba of man.

G, the active form; H, the small precystic form, which will encyst; I, the cyst with a single nucleus and vacuole, which contains a substance (glycogen) which stains brown with iodine.

J–L *Endolimax nana*, a small harmless amœba of man.

J, the active form; K, the precystic form; L, the cyst with four nuclei.

M–N *Dientamœba fragilis*, another small harmless amœba of man, of which the encysted form has not yet been discovered. It frequently has two nuclei.

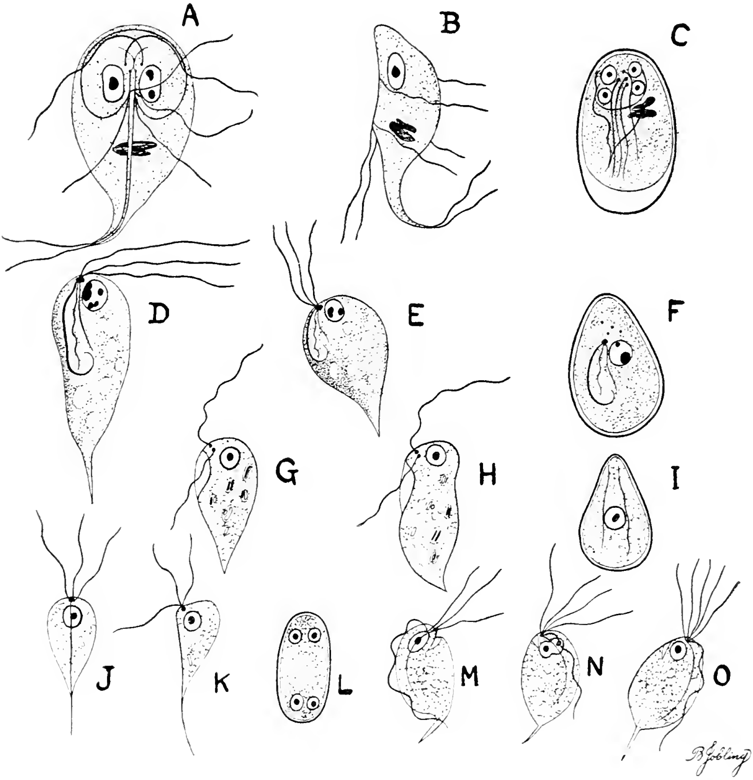

Fig. 2.—The Intestinal Flagellates of Man, which, like the Amœbæ, are carried from man to man by cysts which are passed from the intestine. (Magnified 2000 times.)

A–C *Giardia intestinalis*, which lives in the small intestine.

A, surface view of the flagellate, showing two nuclei, outline of sucker and eight flagella and their connections; B, side view of flagellate; C, encysted form with four nuclei.

D–F *Chilomastix mesnili*, which lives in the large intestine.

D, flagellate with three flagella, large cytostomal groove containing a fourth flagellum and supported by two marginal filaments and a single nucleus; E, smaller flagellate ready for encystment; F, encysted form.

G–I *Embadomonas intestinalis*, which probably lives in the large intestine.

G and H, two views of the flagellate, showing two flagella, cytostomal groove, and a single nucleus; I, encysted form.

J–L *Tricercomonas intestinalis*, which also probably inhabits the large intestine.

J, surface view of the flagellate, showing three anterior flagella, and a fourth one which passes over the surface of the body to become a free flagellum

strains of parasites, subjects of great interest into which there is no time to enter now. Though the majority of natural hosts may suffer little, owing to the delicate mechanisms which render such a balance possible, occasionally a natural host is defective or the strain of parasite introduced is more virulent, so that death results rapidly and host and parasite are destroyed. Such exceptions are disadvantageous to host and parasite alike, but do not invalidate the general principles underlying the relationship of host to parasite.

The microscopic animal parasites, or protozoa, live and multiply in the body of their hosts; it may be in the lumen of the intestine, in the blood stream, or actually in the tissues. It is ultimately necessary for them to gain access to a new host, and this is done in one of two ways. There are produced certain resistant forms which are protected by impermeable and resistant coverings or cysts which enable the parasites to pass long periods outside the body, either on the ground or in water, where they are carried about casually till they are accidentally eaten by another host; or special forms of the parasite are developed, and these are taken up directly by some blood-sucking invertebrate in which they survive and multiply till they are finally transferred to another victim. The majority of microscopic animal parasites adopt the first method of transmission, which may be regarded as the more primitive one. Most, if not all, of the intestinal protozoal parasites of man multiply in the intestine and eventually produce forms protected by cysts. These escape from the intestine in the dejecta, and are disseminated in various ways by water and flies till they are accidentally eaten by some other human beings. The parasite then comes out of its cyst and establishes itself in the intestine.

In the course of evolution the second type of transmission has arisen. It is evident that if intestinal parasites limit themselves to the intestine it is easy for the transmission forms in the encysted condition to gain the lumen of the intestine and find their way to the exterior; but if the parasite changes its habitat, and instead of living in the intestine, or the wall of the intestine, it selects the blood, it is much more difficult for the transmission forms to find their way to the intestine again. On the other hand, being in the blood they can escape from the host in another manner—namely, by way of blood-

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at the posterior end of the body, and a single nucleus; K, side view of the flagellate; L, encysted form, showing four nuclei.

M–O *Trichomonas hominis*, which lives in the large intestine and sometimes in the lower part of the small intestine.

M, form with three flagella (Tritrichomonas); N, form with four flagella (Tetratrichomonas); O, form with five flagella (Pentatrichomonas).

There is a small cytostome or mouth, a single nucleus, an axial rod called the axostyle, which protrudes from the posterior end of the body, and another flagellum which runs along the border of an undulating membrane which is supported by a fibre. The encysted forms are not known.

sucking invertebrates. Having been taken into the intestine of such an invertebrate they may pass unchanged through its intestine and come into the outside world, where they wait till they are eaten by another vertebrate host. But having once entered the invertebrate their chance of survival will be greater if, instead of simply passing through its intestine in a casual manner, they establish themselves and multiply as true parasites. They may produce resistant forms which are passed continuously in the dejecta so often deposited on the skin. It is evident that in this position, as, for instance, on contaminated fingers, it is easy for them to be eaten by the vertebrate. Or they may so regulate their lives in the invertebrate that they come into association with the biting organs, and are inoculated to another vertebrate when a second feed of blood is taken later on.

The parasites which are taken up from the blood by blood-sucking invertebrates can therefore re-enter another vertebrate by two methods. They may find their way to the structures which are related to the biting organs and be inoculated when biting again takes place, or they may appear in the dejecta of the invertebrate and enter the body as a result of co: tamination of the skin. The former is known as the inoculative, and the latter as the contaminative method of infection.

It will be evident that as the habits of invertebrates vary considerably, unless the parasites adapt themselves, or rather become adapted by the process of survival of the fittest, to those particular habits, their chances of transmission are very small indeed, and I want to show by a few examples how this kind of adaptation takes place.

The best known illustration is to be found in the case of the parasites of malaria of man which are transmitted by anopheline mosquitoes. The malarial parasites of man were probably derived in the first place from intestinal parasites, like coccidia, which gave up a life in the wall of the intestine for one in the red blood corpuscles. They multiply in the blood, and eventually produce resistant forms which in their original intestinal habitat would have escaped to the exterior with the dejecta. As these resistant forms are now in the blood it is difficult, if not impossible, for them to get into the intestine, while it constantly happens that they are taken up by blood-sucking mosquitoes. Accordingly they have adapted themselves to a life in the mosquito, but instead of passing through the mosquito's intestine to the exterior, as might have been the case, and possibly did occur, during the course of evolution, they multiply in the mosquito's body and produce a large number of small sickle-shaped bodies, known as sporozoites, which are scattered all through the tissues. Now the mosquito, in common with other insects which feed on blood, has the habit of injecting saliva from its salivary glands into the wound at the commencement of its feed. It is probable that this saliva, which is an irritating fluid, causes the blood supply to the area around the wound to be increased so that the mosquito can more readily obtain the blood it desires. The sporo-

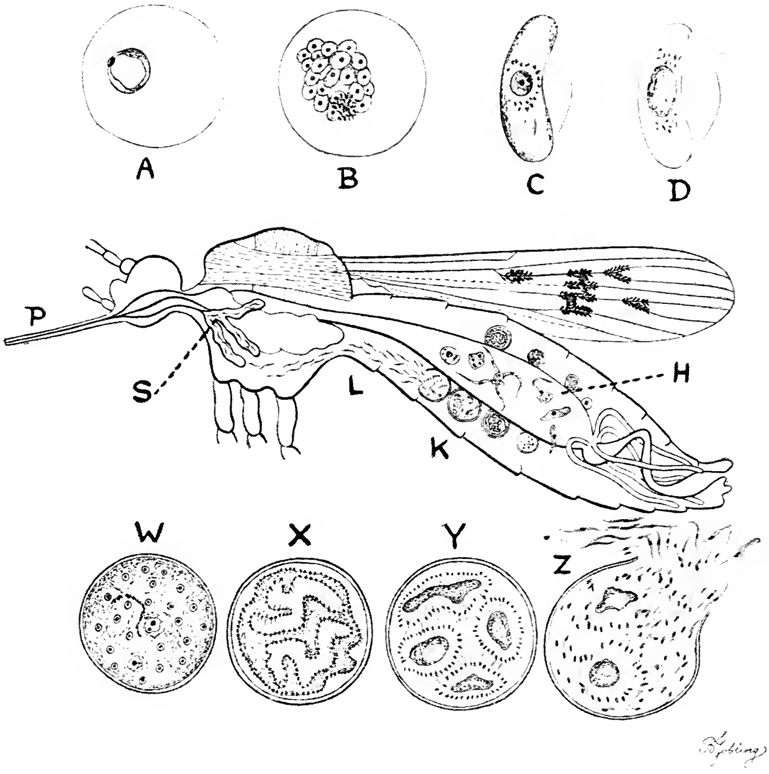

FIG. 3.—*Plasmodium falciparum*, THE MALIGNANT TERTIAN MALARIAL PARASITE OF MAN, AND ITS DEVELOPMENT IN THE ANOPHELINE MOSQUITO.

A–D The parasites in the red blood corpuscles of man. (Magnified 3000 times.)

A, young ring form; B, adult multiplying form; C and D, female and male crescent forms (gametocytes), which can only develop further in the mosquito.

P, S, L, K, H Diagram of development in the mosquito.

H, development in the lumen of the stomach; K, development on the outer surface of the stomach; L, sporozoites escaping from cyst and making their way towards the salivary glands; S, salivary glands containing sporozoites; P, proboscis, which inflicts the wound and through which the saliva containing sporozoites is injected.

W–Z Details of the development of the cysts on the stomach of the mosquito as shown at K. (Magnified 400 times.)

zoites of the malaria parasites, if they can get into the salivary glands, will have every chance of being inoculated to the man on whom the infected mosquito feeds. This is exactly what happens, and the malarial parasite has, so to speak, availed itself of the habit the mosquito has of injecting saliva in order to enter another host. At the present time the fact that some of the sporozoites find their way to the salivary glands seems to be purely fortuitous, for sporozoites are found in other parts of the mosquito's body also, such as the legs and antennæ, and these cannot possibly get into man unless we suppose that the mosquitoes themselves are eaten by man, a very unlikely thing to happen. There is thus a great waste of sporozoites from the point of view of the malarial parasite. It may come about in course of time that a race of malarial parasites will be evolved which produces sporozoites having a special affinity for the salivary glands. It is evident that only those which enter the salivary glands have any chance of further development in man, and if the quality which causes attraction between parasite and salivary gland be hereditarily transmitted to subsequent generations of parasites, a race may be evolved which produces sporozoites with this character more highly developed than at the present time, so that from the point of view of the parasite there will be less waste of sporozoites. When the mosquito has ejected all the sporozoites which have developed from one batch of parasites taken up from the blood, or when any sporozoites remaining in the salivary glands have degenerated and died, the mosquito ceases to be infective.

Another illustration of the same type of development is seen in the case of the tse-tse fly and the trypanosomes causing sleeping sickness of man, nagana of domestic animals, and other diseases. These blood-inhabiting flagellates may originally have been purely insect parasites, which lived in the intestine, and passed from one insect to another by means of encysted forms which escaped in their dejecta, just as the intestinal parasites of man pass directly from one man to another by encysted forms which escape in the fæces; or they may have been, as some authorities hold, intestinal parasites of man, or some other vertebrate, which, having invaded the blood stream, have secondarily become adapted to an insect, as apparently occurred in the case of the malarial parasites. Whichever view is correct, and the former seems to me to be the more probable, at the present time the trypanosomes live in the blood of man and animals in Africa, and are handed on from man to man by tse-tse flies. In these flies the trypanosomes undergo an evolution which differs in many respects from that of the malarial parasites in mosquitoes, but resembles them in the ultimate result. The trypanosomes make their way to the salivary glands of the tse-tse flies, and as these insects have the same irritating habit of injecting saliva when they feed, the trypanosomes are easily inoculated into man. It has been estimated that as many as five thousand trypanosomes can be injected

by a single fly during one feed. It is probable that when once infected the fly remains so for the rest of its life.

In the case, therefore, of both the malarial parasites and those of sleeping sickness and allied diseases, the habit of the insects of injecting saliva when they feed has been utilised by the parasites in order to reach the necessary host and to maintain its species.

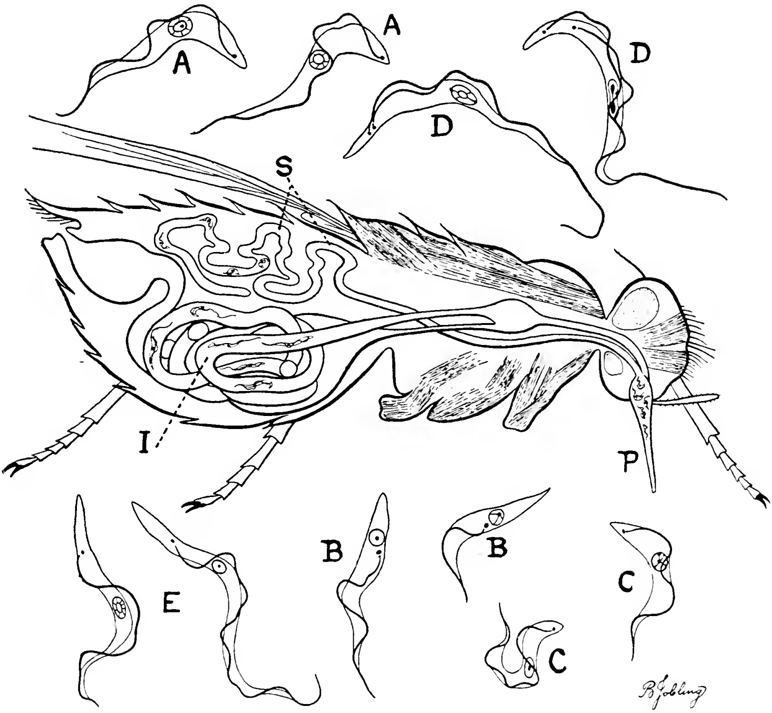

Fig. 4.—*Trypanosoma gambiense*, the Trypanosome of Sleeping Sickness of Man, and its development in the tse-tse fly (*Glossina palpalis*).

A–D Trypanosomes in the blood of man. (Magnified 1500 times.)

A, typical trypanosomes; D, fully grown dividing forms.

I, P, S Diagram of development in the tse-tse fly.

I, trypanosomes in the stomach; S, trypanosomes which later enter the salivary glands; P, proboscis which inflicts the wound and from which the trypanosomes of the salivary glands are injected into man.

B, C, E Development in the tse-tse fly which terminates in the infective forms C, which are the ones which infect man, and are produced in the salivary glands from crithidial forms B, which result from trypanosome forms E which have made their way from the intestine to the salivary glands. (Magnified 1500 times.)

Though trypanosomes give rise to sleeping sickness in man and produce similar diseases in domestic animals, these hosts, as I explained before, are not to be regarded as the natural hosts, which are actually the wild game upon which tse-tse flies also feed. Similar trypanosomes occur in many other animals, and they are commonly

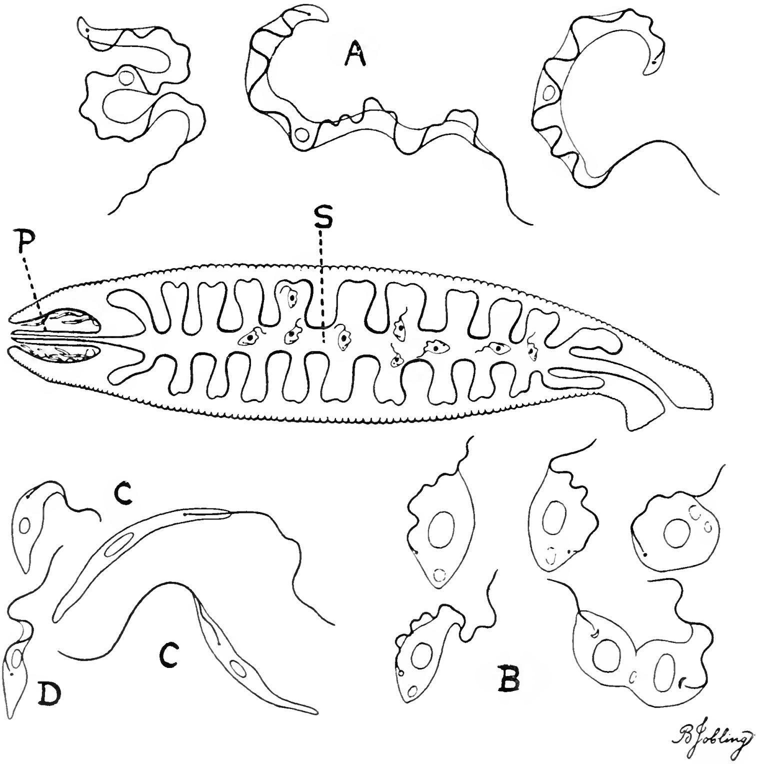

FIG. 5.—A TRYPANOSOME OF A FISH, AND ITS DEVELOPMENT IN A LEECH.

A Trypanosome in the blood of the fish. (Magnified 1500 times.)

P–S Diagram of the development in the leech.

S, stomach of the leech containing multiplying trypanosomes; P, proboscis of leech surrounded by the cavity of the proboscis sheath containing infective trypanosomes which have made their way there from the stomach by passing through the mouth at the end of the proboscis.

B, C, D Development of the trypanosome in the leech. (Magnified 1500 times.)

B, developmental forms in the stomach; C, elongate forms in the proboscis sheath; D, trypanosome form which enters the fish through the wound made by the proboscis.

present in fish, frogs and other cold-blooded aquatic vertebrates. It is clear that the trypanosomes of fish cannot be conveyed from fish to fish by biting insects. Another blood-sucking invertebrate, the leech, is responsible for their transmission. When the leech feeds it does so by means of a proboscis armed with teeth, and this proboscis is lodged in a sheath or pouch, through the opening of which it can be protruded. The trypanosomes have availed themselves of the presence of this proboscis sheath. Taken into the stomach of the leech when it feeds on an infected fish or frog, the trypanosomes multiply there, and finally migrate forwards along the œsophagus and proboscis. They pass out of the mouth at the end of the proboscis and enter the proboscis sheath, where they continue to multiply and wait their opportunity. When the leech feeds the circular opening of the proboscis sheath is placed against the skin of the fish, and the proboscis with its teeth inflicts the wound. The fluid in the proboscis sheath, which may be swarming with trypanosomes, comes in contact with the wound and the trypanosomes enter the body of the fish. The trypanosomes in this instance have not availed themselves of the salivary glands but of the sac-like proboscis sheath.

In the three examples I have given it will be seen that infection is spread by the biting parts of the invertebrate, and that the parasites develop in the anterior region of the body. This type of development has been termed a development in the anterior station. In the examples which I will now give the development is at the opposite end of the body—a development in the posterior station—and the method of transmission is correspondingly different.

The common rat very frequently harbours a trypanosome in its blood which is conveyed from rat to rat by fleas. Now, it might have arisen that these trypanosomes would avail themselves of the salivary glands of the flea, as in the case of the tse-tse fly, but this is not so. The flea has the peculiar habit, which is shared by many insects, of not ceasing to feed when its stomach is full. In order to accommodate the blood which it is greedily sucking it has constantly to empty its intestine. If one watches a flea feed it will be noticed that every few minutes it ejects from its hinder end considerable quantities of blood. In this manner a flea is able to enjoy a prolonged feed which may continue for an hour or more. Hence the numerous spots on the sheets if one is unfortunate enough to have to sleep with one of these insects.

The trypanosome of the rat, when taken up by the flea, multiplies in the flea's intestine, and finally passes backwards to the rectum, where multiplication is continued. When such a flea feeds, every time it ejects a droplet of blood some of the trypanosomes in the rectum are carried with it, and are deposited on the skin or fur of the rat. If a white rat which is infested with fleas is watched it will be seen that the insects tend to congregate on the back of the rat above the tail. Such a rat will go to sleep, and the fleas will seize

the opportunity of indulging in a feed of blood while the rat is off its guard. The white fur will become bespattered with droplets of blood ejected by the fleas. The rat, suddenly aroused from its torpor, turns its head round to alleviate the irritation by licking the spot, in doing which it laps up the blood which the fleas have deposited.

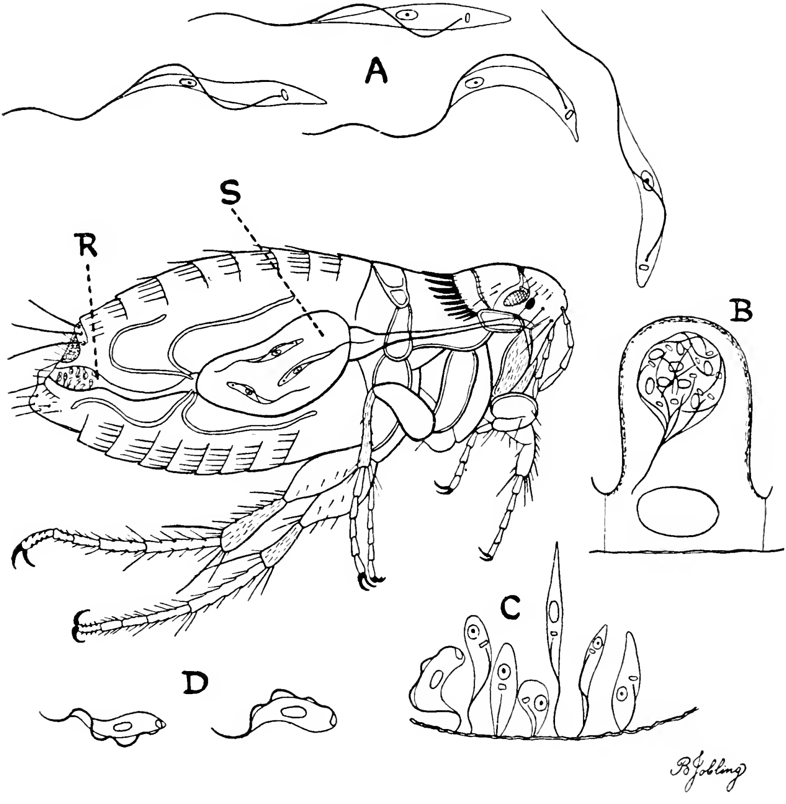

FIG. 6.—THE TRYPANOSOME (*Trypanosoma lewisi*) OF THE RAT, AND ITS DEVELOPMENT IN THE FLEA.

A Trypanosomes in the blood of the rat. (Magnified 1500 times.)

R–S Diagram of the development in the flea.

S, trypanosomes in the stomach; R, trypanosomes in the rectum which are deposited on the skin in the droplets of blood ejected by the flea.

B, C, D Details of development in the flea. (Magnified 1500 times.)

B, multiplying trypanosome in one of the cells lining the stomach of the flea; C, developmental forms attached to the cells lining the rectum of the flea; D, trypanosome forms which are voided in the dejecta of the flea and which are eaten by the rat and cause its infection.

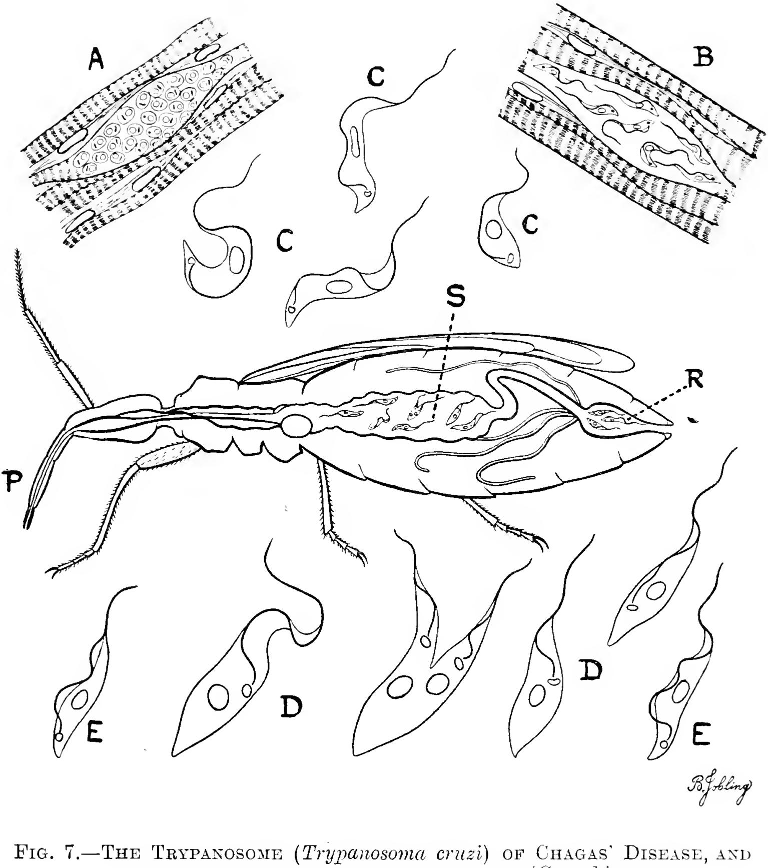

FIG. 7.—THE TRYPANOSOME (*Trypanosoma cruzi*) OF CHAGAS' DISEASE, AND ITS DEVELOPMENT IN THE REDUVIID BUG (*Conorhinus* (*Triatoma*) *megistus*).

A, B, C Development of the trypanosome in the blood and tissues of man.

A, multiplication of small round forms in a muscle-fibre of the heart; B, transformation of the small round forms into trypanosomes; C, trypanosomes which have escaped from the ruptured muscle-fibre into the blood. (A and B magnified 500 times, C magnified 1500 times.)

P, R, S Diagram of the development in the reduviid bug.

S, trypanosomes in the stomach of the bug; R, rectum containing trypanosomes which are deposited on the skin, and lead to the infection of man by contamination.

D–E Development of the trypanosome in the bug. (Magnified 1500 times.)

D, multiplying crithidial forms; E, trypanosome forms which produce the infection in man.

Fleas which have become infected on one rat will wander from rat to rat, and in a short time the infection will spread. Very careful experiments have been made which prove that the actual bite of the flea does not convey the infection. Fleas can be studied very conveniently by fixing them on fine wire passed round the thorax, as is done by showmen. A flea so tethered can be easily handled and its movements controlled, while the droplets of blood which it ejects while feeding can be received on to slides and examined with the microscope. Such a flea I have kept alive for about four months. A tethered flea can be fed on a rat in the blood of which trypanosomes are present. Subsequently it can be fed on one's own wrist, and it will be found that after the expiry of about six days the droplets of ejected blood will contain small trypanosomes which have become established in the rectum of the flea. The insect is now in the infective condition. The flea can be placed on an uninfected rat, and while it is feeding care can be taken to protect the skin so that no droplets of blood are allowed to fall upon it. The droplets can be received on to a cover-glass held behind the flea. With a fine pipette the droplets can be sucked up from the cover-glass and introduced into the mouth of another uninfected rat This experiment was repeated many times with different species of flea—the rat flea, the dog flea, the human flea, and the Indian plague flea—always with the same result. The rat on which the fleas actually fed never became infected, whereas those which received the droplets in the mouth did so. It is thus clear that the infection is spread, not by the bite of the flea, but by the droplets of infective blood which the flea ejects from its rectum. The trypanosome of the rat has availed itself of the gluttonous habits of the flea in order to effect its transmission from rat to rat. The flea continues to void trypanosomes for the remainder of its life.

A trypanosome which produces a disease of human beings in certain parts of South America resembles the harmless trypanosome of the rat in the mechanism of its conveyance. This form is peculiar in that within the vertebrate host it multiplies chiefly within the muscle-fibres, but also in other organs like the thyroid gland, as small rounded bodies which finally grow into trypanosomes which invade the blood. Other trypanosomes, like those of sleeping sickness and the harmless one of the rat, multiply in the trypanosome form while they are swimming about in the blood stream. In the case of the South American disease, or Chagas' disease, as it is called after its discoverer, the trypanosomes which have developed in the muscle-fibres make their way into the circulating blood, whence they are sucked up by a large reduviid bug which lurks in the crevices of wood and comes out at night to bite its victims. The trypanosomes multiply in the intestine of the bug, and, as in the case of the rat trypanosome, they finally establish themselves in the posterior portion of the intestine or rectum. From this situation they are voided in

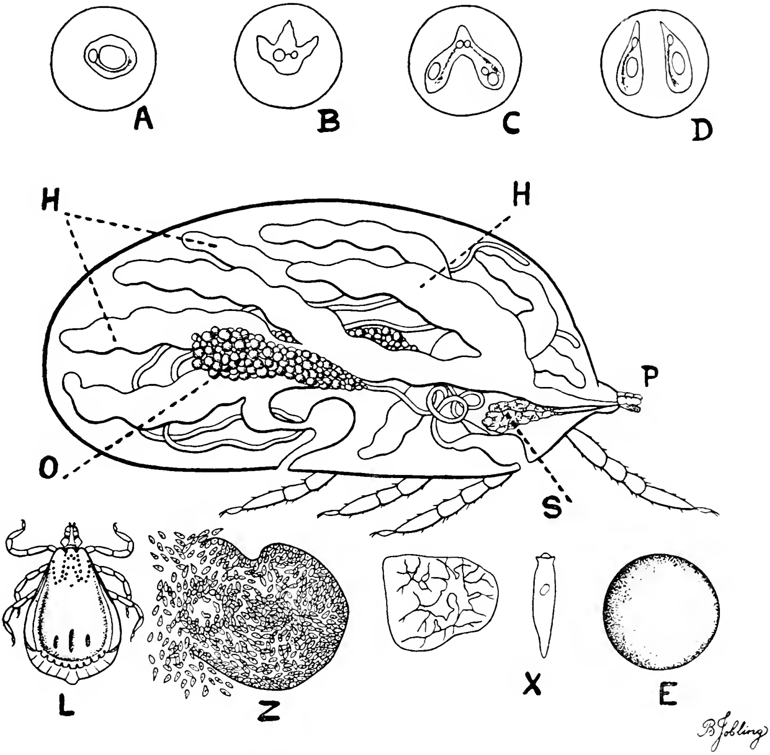

FIG. 8.—PIROPLASMA (*Babesia canis*) OF THE DOG, AND ITS TRANSMISSION BY THE DOG TICK (*Rhipicephalus sanguinæus*).

A–D Parasites in the red blood corpuscles of the dog. (Magnified 2000 times.)
A–B, two forms of the parasite, the round and the amœboid form; C–D, multiplication in the blood by division of the parasite into two.

H, O, P, S Diagram of the tick to show the very much branched stomach, H; the salivary gland, S; the proboscis, P; and the ovary, O.

E Egg of tick, which contains the parasite.

L Larva of tick, which is infected when it hatches from the egg.

X–Z Development of the parasite in the tick. (Magnified 3000 times.)
X, elongate form which grows in the tissues of the tick, and finally produces a large number of minute parasites, Z, which invade the ovaries and infect the eggs.

the fæces which the bug passes when it feeds. This material containing trypanosomes is either rubbed or scratched into the wound inflicted by the bug, or is carried on the fingers to the mouth in such a way as to bring about infection of the human being. The bug remains infective for the rest of its days.

We thus find from the examples we have just been considering that the parasites in the invertebrate tend to develop in the anterior station when infection is to be inoculative, and in the posterior station when it is contaminative.

There is another class of blood-sucking invertebrates which differ from those we have been talking about in that they do not feed repeatedly. A mosquito or a tse-tse fly feeds possibly every day, while in the intervals between its feeds it lives elsewhere. Now in the case of ticks there is a different state of affairs. The young tick hatched from the egg fixes itself to the skin of its host by its proboscis, and may remain there for the rest of its days, only loosening its hold towards the end of its life to fall on to the ground, where it lays eggs and dies. Some ticks pass their larval stage on one vertebrate, the nymph stage on a second, and the adult stage on a third. Others have only two hosts. The ticks are known, therefore, as one-host ticks, two-host ticks and three-host ticks. In the case of a one-host tick if a parasite were to avail itself of this host it might at first sight seem impossible for such a tick to transmit an infection, for it lives on but one host, and leaves it only to lay eggs and die. It does not visit a second host to which it might convey a parasite which it had taken up. But we shall see that such a transmission is still possible. There is a group of parasites known as piroplasmata which in many respects resemble the malarial parasites of man. They live in the red blood corpuscles, where they multiply. They do not, however, produce the characteristic brown pigment which is found in the malarial parasites. They occur in cattle, horses, sheep, dogs and other animals, and give rise to serious diseases known as red-water fever and malignant jaundice. These organisms are taken up from the blood by ticks, where they multiply and finally invade the ovaries. When the tick leaves its host and lays its eggs the latter have already the parasites within them, so that the young ticks or larvæ which develop in the egg become infected. The parasites multiply in the young ticks, and when they attach themselves to another host they transmit the infection. It is not yet clear whether the parasites are injected with the saliva of the tick, or whether they are deposited on the skin in its dejecta, or in secretions from certain glands; but the interesting fact is that the parasite has adapted itself to a life in the egg of the tick so that it may be transmitted by the succeeding generation. In the case of two-host ticks and three-host ticks this passage of the parasite through the egg still takes place, but it also happens that the parasites taken up by the first stage or larva of the tick may be transmitted when one of the succeeding

stages become attached to another host. In addition to the piroplasmata, ticks are also responsible for the conveyance of spirochætes of relapsing fever, and in this case also the parasites pass through the egg to the succeeding generation, and, even though the ticks hatched from the egg never have an opportunity of taking up spirochætes from the blood again, their offspring may also be infective. In other words, the infection may pass through several generations of tick. The spirochætes of relapsing fever live in the blood, and though in tropical Africa, South and Central America, and some other parts of the world they are transmitted by ticks, in Europe and Asia

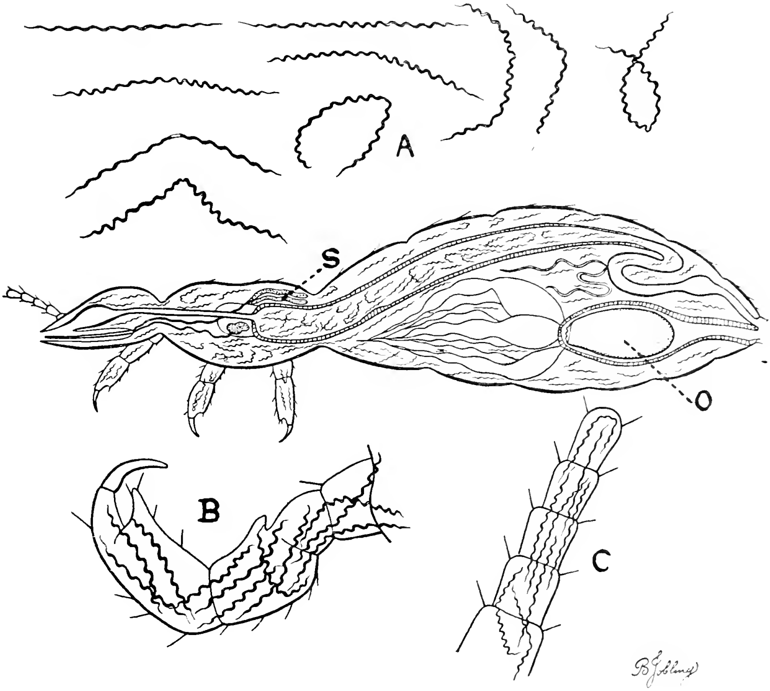

FIG. 9.—THE SPIROCHÆTE (*Spironema recurrentis*) OF RELAPSING FEVER, AND ITS DEVELOPMENT IN THE LOUSE (*Pediculus vestimenti*).

A Spirochætes as they occur in the blood of man. (Magnified 2000 times.)

O, S Diagram of the development in the louse, showing the whole body invaded by the spirochætes.

S, salivary glands; O, ovum which may become infected and give rise to infected larvæ.

B, C Spirochætes in the leg and antenna, which are easily broken off, so that fluid exuding from the body infects the skin.

the louse is the culprit. There is some evidence that the spirochæte in the louse may pass through the egg, as in the case of the tick, but there is a more common mode of transmission. Though such a persistent ectoparasite of man under filthy conditions, and though difficult to eradicate when there is poverty and overcrowding, the louse is a very delicate creature which is easily damaged. The spirochætes which the louse has sucked up in the blood multiply rapidly in the body of the louse till all the body fluids are crowded with them. They extend into every part of the body, including the legs and antennæ. The violent and constant scratching in which a lousy individual indulges frequently damages the lice so that their legs are broken off. Fluid filled with spirochætes exudes from the stump of the broken leg on to the skin, and this makes its way into the puncture wound inflicted by the louse, or into the erosions caused by the nails of the unfortunate individual himself. Infection is spread from man to man by the damage caused to the louse. From the point of view of the spirochæte this is a highly satisfactory arrangement, but hardly so for the louse. Still less is the method of transmission which occurs in some cases, as, for instance, that of the hæmogregarine of the dog—a parasite which lives in the white blood corpuscles, and which, like the piroplasma of the same animal, is conveyed by ticks. The hæmogregarines gain entrance to another dog probably by the dog actually devouring the infected tick.

Time prevents me from entering into this most interesting subject in greater detail. I hope I have made it clear how parasites, which originally passed directly from one host to another by means of resistant stages protected by cysts which contaminated water, have, in the course of evolution, changed their habitat in the body, passing from the intestine to the blood, and how it is in consequence of this change that they have been obliged to seek some other means of transit. Quite naturally, blood-sucking invertebrates were utilised to this end, but the behaviour of any parasite in the invertebrate had of necessity to be closely adapted to the particular habits of the invertebrate it employed.

I have spoken throughout of the parasites "availing themselves" of this and that peculiarity, but I have merely done so for the sake of convenience of speech. I do not wish to imply that the parasites are in any way conscious of what is going on. Once in the invertebrate they multiply and spread through the body indiscriminately. It is only those which happen to arrive at a situation whence they can re-enter a host which will be transmitted. It may be that the fortunate ones have some peculiarity, possibly of a chemical nature, which causes them to be drawn there by a chemiotactic action. If this be so, then the offspring of the lucky few will probably inherit this quality to a higher degree, so that the next time they infect the invertebrate a still smaller number will go astray. Finally, there will be developed that peculiarity, so characteristic of parasites

generally, which leads them in some mysterious manner to special parts of the body which they appear to select by instinct—an instinct which may be nothing more than a chemical peculiarity acquired by a process of natural selection.

You will readily understand the great importance of the kind of knowledge which the study of insects in relation to the transmission of disease has given us. The life-history of one of these parasites can be regarded as a circular chain composed of various links. If any one of these links is broken the life of the parasite ceases and the spread of disease is prevented. The protection of infected individuals from the particular invertebrate hosts, the destruction of the invertebrates or the prevention of their breeding, the killing of the parasites in the vertebrate—these and other methods are now in practice in the prevention of malaria, yellow fever, sleeping sickness, and other diseases. Our knowledge of the parasites themselves and of their invertebrate hosts is, however, still defective, and there is much useful work remaining to be done in the realms of Parasitology and Entomology before hygienic measures of this kind can reach perfection.

[C. M. W.]

GENERAL MONTHLY MEETING,

Monday, March 6, 1922.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Miss Alice Bagot,
William Valentine Ball,
Miss Emma Jane Blunt,
Sydney Bryan Donkin,
Reginald Edmund Gibbs,
Geoffrey Norman Edward Hall-Say,
Herbert Antony Hankey,
Miss Anna M. Herron,
Mrs. Aylmer Lloyd,
Dallyn Lucas,
Sir Murdoch MacDonald, K.C.M.G. C.B.
Mrs. Reginald McKenna,
Herbert Guy Moberly,
George S. Odling-Smee, J.P.
Mrs. W. Carpenter Scott,
Richard Seligman,
Mrs. Sharman-Crawford,
Grafton Elliot Smith, M.D. F.R.S.
Arthur Stanley, M.D.
Charles F. M. West,
Edward Yatman,

were elected Members.

The Special Thanks of the Members were returned to Viscount Burnham, J.P., for his Donation of Twelve Pounds to the Fund for the Promotion of Experimental Research at Low Temperatures.

The following Lecture Arrangements After Easter 1922 were announced :—

Sir Arthur Keith, M.D. LL.D. F.R.S. M.R.I., Fullerian Prof. of Physiology, R.I.. Three Lectures on Anthropological Problems of the British Empire: Series II. Racial Problems of Africa. On *Tuesdays*, April 25, May 2, 9.

William Bulloch, M.D. LL.D. F.R.S., Prof. of Bacteriology, University of London. Two Lectures on Tyndall's Biological Researches and The Foundations of Bacteriology. (The Tyndall Lectures.) On *Tuesdays*, May 16, 23.

Sir Percy Sykes, K.C.I.E. C.B. C.M.G. Two Lectures on 1. Twenty-Five Years' Travel in Persia; 2. The Foundation of the Persian Empire. On *Tuesdays*, May 30, June 6.

Edwin H. Barton, D.Sc. F.R.S., Prof. of Experimental Physics, University College, Nottingham. Two Lectures on Audition and Colour Vision: 1. The Resonance Theory of Audition; 2. A Syntonic Hypothesis of Colour Vision. On *Thursdays*, April 27, May 4.

Frederick Keeble, O.B.E. Sc.D. F.R.S., Prof. of Botany and Fellow of Magdalen College, Oxford. Two Lectures on Plant Sensitiveness: 1. To Light; 2. To Contact and to Chemical Stimulation. On *Thursdays*, May 11, 18.

The Very Rev. William Ralph Inge, D.D., Dean of St. Paul's. Three Lectures on Theocracy: 1. Theocracies in General; 2. The Mediæval Idea; 3. The State Invisible. On *Wednesday*, May 24, *Thursdays*, June 1, 8.

Professor D. H. MacGregor, M.A. M.C., Prof. of Political Economy, University of Oxford. Two Lectures on Industrial Relationship: 1. The Historical Interpretation; 2. The Problem of Structure. On *Wednesday*, April 26, *Saturday*, May 6.

O. W. Richardson, D.Sc. F.R.S., Prof. of Physics, King's College, London. Two Lectures on The Disappearing Gap between the X-Ray and Ultra-Violet Spectra: 1. Grating Results; 2. Photo-Electric Methods. On *Saturdays*, May 13, 20.

Sir Hugh Allen, M.A. Mus.Doc., Director of Royal College of Music. Three Lectures (with Musical Illustrations by Harold Samuel) on Early Keyboard Music. On *Saturdays*, May 27, June 3, 10.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal of India, Vol. XVII. No. 1. 8vo. 1922.
American Philosophical Society—Proceedings, Vol. LX. No. 2. 8vo. 1921.
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Beck, Conrad, Esq. (The Author)—The Microscope. 8vo. 1921.
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British Architects, Royal Institute of—Journal, Third Series, Vol. XXIX. Nos. 7–8. 4to. 1922.
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Applications of Chemistry to Crop Production. By E. J. Russell. 8vo. 1922.
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Dale, H. B., Esq. (The Author)—Landmarks in Armenian History. 8vo. 1922.
Dunk, J. L., Esq. (The Author)—Hyperacoustics: Division II. Successive Tonality. 8vo. 1921.

Editors—Animals' Defender, March 1922. 8vo.
Beama, Jan. 1922. 8vo.
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Chemist and Druggist, Feb. 1922. 8vo.
Dyer and Calico Printer, Feb. 1922. 4to.
Engineer, Feb. 1922. fol.
Engineering, Feb. 1922. fol.
Ferro-Concrete, Feb. 1922. 8vo.
General Electric Review, Feb. 1922. 8vo.
Good Housekeeping, March 1922. 8vo.
Journal of Physical Chemistry, Dec. 1921. 8vo.
Junior Mechanics, Feb. 1922. 8vo.
Law Journal, Feb. 1922. 8vo.
Model Engineer, Feb. 1922. 8vo.
Musical Times, Feb. 1922. 8vo.
Nation and Athenæum, Feb. 1922. 4to.
Nature, Feb. 1922. 4to.
New Church Magazine, March–April 1922. 8vo.
Nuovo Cimento, Jan. 1922. 8vo.
Physical Review, Jan. 1922. 8vo.
Science Abstracts, Jan. 1922. 8vo.
Wireless World, Feb. 1922. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LX. No. 306, Feb. 1922. 8vo.
List of Members, 1921. 8vo.
Franklin Institute—Journal, Vol. CXCIII. No. 2. 8vo. 1922.
Gauthier-Villars et Cie. (The Publishers)—La Théorie de la Relativité, par E. Picard. 8vo. 1922.
L'Éther Actuel et ses Précurseurs, par E. M. Lemeray. 8vo. 1922.
Geographical Society, Royal—Journal, Vol. LIX. No. 2. 8vo. 1922.
Geological Society of London—Abstracts of Proceedings, Nos. 1081-83. 8vo. 1922.
Hall, W., M.A. (The Author)—Poems of a Riper Experience. 8vo. 1921.
Horological Institute—Journal, Feb. 1922. 8vo.
Illuminating Engineering Society—Illuminating Engineer, Vol. XIV. No. 10. 8vo. 1921.
Kramer, J. B., Esq. (The Author)—Radiations from Slow Radium. 8vo. 1921.
Linnean Society—Journal, Zoology, Vol. XXXIV. No. 230. 8vo. 1922.
Liverpool Literary and Philosophical Society—Proceedings (1918–1921), No. LXVI. 8vo. 1921.
London County Council—Gazette, Feb. 1922. 4to.
London Society—Journal, Feb.–March 1922. 8vo.
London University—Gazette, Feb. 1922. 4to.
Meteorological Society, Royal—Journal, Vol. XLVIII. No. 201, Jan. 1922. 8vo.
Mitzakis, M., Esq. (The Author)—The Oil Encyclopedia. 8vo. 1922.
Mohamed Afzal, Khan Bahadur (The Author)—Chronogrammatic Poems in Persian on Visit of H.R.H. the Prince of Wales to India (with Translation). 4to. 1921.
Montpellier Académie des Sciences—Bulletin, April–Dec. 1921. 8vo. 1922.
National Academy of Sciences, Washington—Proceedings, Vol. VII. Nos. 11–12. 8vo. 1921.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, Dec. 1921. 8vo.
Paris, Société Française de Physique—Journal de Physique et le Radium, Tome III. No. 1. 8vo. 1922.
Peru, Corps of Mining Engineers—Anales de la Industria Minera, Anexo. 8vo. 1921.
Boletin, No. 101. 8vo. 1921.

Pharmaceutical Society of Great Britain—Journal, Feb. 1922. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLVI. No. 3. 8vo. 1922.
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Queensland Museum—Memoirs, Vol. VII. Part 3. 8vo. 1921.
Rome, Ministry of Public Works—Giornale del Genio Civile, Nov.–Dec. 1921. 8vo.
Royal Engineers' Institute—Journal, Vol. XXXV. No. 3. 8vo. 1922.
Royal Society of Arts—Journal, Feb. 1922. 8vo.
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Year Book, 1922. 8vo.
South Africa, The High Commissioner for—Official Year Book of the Union of South Africa, No. 4, 1910–1920. 8vo. 1921.
Journal of Agriculture, Vol. IV. No. 2. 8vo. 1922.
Statistical Society, Royal—Journal, Vol. LXXXV. Part 1. 8vo. 1922.
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Taylor, F. Coston, Esq., M.A. M.R.I.—Print of Richard Arkwright's Advertisement (*circa* 1755–1767). (From original plate.)
Tôhoku Imperial University—Technology Reports, Vol. II. No. 3. 8vo. 1921.
Science Reports, Vol. X. No. 5. 8vo. 1921.
Mathematical Journal, Vol. XX. Nos. 1–2. 8vo. 1921.
United States Bureau of Standards—Scientific Papers, Nos. 413–416, 418–420. 8vo. 1921.
Circulars, No. 8 (3rd Ed.), 112, 114, 116. 8vo. 1921.
Technologic Papers, Nos. 194–195. 8vo. 1921.
United States Patent Office—Official Gazette, Vol. CCXCIV. No. 4—Vol. CCXCV. No. 2. 8vo. 1922.
Yorkshire Archæological Society—Journal, Vol. XXXVI. Part 3. 8vo. 1922.
Zoological Society—Proceedings, 1921, Part 4. 8vo. 1922.
Zurich Naturforschenden Gesellschaft—Vierteljahrsschrift, 1921, Heft 3–4. 8vo.

WEEKLY EVENING MEETING,

Friday, March 10, 1922.

Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S.,
Secretary and Vice-President, in the Chair.

Thomas R. Merton, M.A. D.Sc. F.R.S., Professor of Spectroscopy in the University of Oxford.

Problems in the Variability of Spectra.

[Abstract.]

It has been known for many years that the radiations which an element emits in the state of a luminous gas are not invariable, but depend on the presence of other elements, the manner in which the substance is excited to luminosity, and other circumstances. It was recognised in some of the earliest investigations that many band spectra were to be associated with compounds, and that a spectrum might be due partly to such compounds and partly to uncombined atoms. Thus, for example, if strontium chloride is introduced into the flame of the Bunsen burner we find lines associated with the element, bands due to strontium oxide, and also bands due to the chloride, and when strontium bromide is substituted for the chloride the spectrum is the same as regards the lines due to the element and the oxide bands, but bands peculiar to the bromide are found to have replaced those due to the chloride. Minute quantities of substances can sometimes be detected by means of these characteristic bands due to compounds, a familiar example being the blue flame which is seen when common salt is thrown on to a coal fire, and which is due to the copper chloride formed from the chlorine in the common salt and the minute trace of copper which is present in the coal. A number of different elements are present in most flames, and the reactions which occur are probably very complex. In gases contained in vacuum tubes which are excited to luminosity by electrical discharges, it is possible to work with pure substances, and a discussion of the spectra observed is simpler.

In the case of gases in vacuum tubes the spectrum sometimes consists of bands, and the band spectrum from the negative pole may be different from that seen in the positive column. Thus nitrogen, when excited by uncondensed discharges, shows in the visible

regions two band spectra—one known as the positive band spectrum, which appears in the capillary of a vacuum tube of the conventional type; and the negative band spectrum, which is found in the neighbourhood of the cathode, and which constitutes an important part of the spectrum of the aurora. Both these band spectra, and indeed all band spectra, are generally attributed to molecules rather than atoms; but if a condensed discharge is passed through nitrogen the spark spectrum associated with the nitrogen atom is obtained, and this is capable of further modification when discharges of great intensity are employed. The action of the condensed discharge is almost certainly due to the greatly increased current density which obtains during the very brief periods while the discharge is passing, and its first effect is to break up the molecules into atoms, and the further stages brought about by an increase in the intensity of the discharge are generally supposed to be due to the removal of successive electrons from the atoms. There are other methods by which the current density can be increased with similar changes in the spectrum, the effect of an increase in the current density being to increase the number of charged particles in a given volume of the gas, with the result that a large number of the radiating atoms are subjected to intense electric fields due to neighbouring charged particles.

Similar results are observed in the spectra associated with carbon. There are at least six spectra due to compounds of carbon with hydrogen, oxygen and nitrogen, and special experimental conditions are necessary for the production of some of these spectra. In addition to these band spectra carbon shows line spectra, and with the most intense discharges which can be employed in the laboratory a number of new lines appear which are also found in the spectra of the hottest type of stars, known as the Class O, or Wolf-Rayet stars. All these changes can be reasonably accounted for, but there are a number of other changes which are more difficult to explain. For many reasons the spectrum of hydrogen is of particular interest, because the atom of hydrogen is the simplest known atom, and is supposed to consist of a positive nucleus and a single electron. There are two spectra associated with hydrogen, one of which is known as the Balmer series and is found in almost all celestial spectra, and also in vacuum tubes in the laboratory unless the most rigorous precautions are taken to exclude all traces of hydrogen. The explanation of the origin of this spectrum has been one of the most striking successes of the quantum theory of spectra developed by Bohr and by Sommerfeld. The other spectrum of hydrogen, known as the secondary spectrum, consists of an enormous number of lines, and differs in its mode of production from the Balmer series in that the secondary spectrum is characteristic of pure hydrogen. In the purest hydrogen obtainable the secondary spectrum may be as bright as the Balmer series, but if the smallest trace of impurity is present the Balmer series gains in intensity and the secondary spec-

trum becomes very much weaker. In a vacuum tube containing water vapour the lines of the Balmer series are extremely intense, whilst those of the secondary spectrum are relatively very faint. The investigations of Michelson and Lord Rayleigh, and of Buisson and Fabry, have shown that under certain conditions the masses of the atoms or molecules from which the spectrum originates may be deduced from a knowledge of the widths of the spectrum lines, and recent investigations, in which the widths of the lines of the secondary spectrum of hydrogen have been measured to a high degree of precision, have shown that the secondary spectrum is to be referred to the hydrogen molecule. The presence of impurities in vacuum tubes containing hydrogen not only enhances the lines of the Balmer series, but also brings about changes in the relative intensities of the Balmer lines themselves. Some of these changes are very striking, but there are other variations of a more subtle kind, which are only discovered when accurate quantitative measurements are made of the relative intensities of the lines. A most striking effect is observed when a relatively large quantity of helium is admitted to a vacuum tube containing hydrogen. Under these conditions the relative intensities of some of the lines of the secondary spectrum alter in a surprising manner, some of the lines being greatly enhanced, whilst others become very weak.

From a theoretical point of view the spectrum of helium is second in importance to that of hydrogen only. The lines of helium are prominent in the spectrum of the chromosphere of the sun and of many stars, and their relative intensity varies under different conditions of excitation in the laboratory and in different celestial spectra. There are six chief series of lines in the spectrum of helium, three of which are usually referred to as the helium and three as the parhelium series. The helium series are the stronger in vacuum tubes containing the gas at pressures exceeding a few millimetres, whilst at very low pressures the parhelium series are predominant; and since the chief visible line of the helium series is yellow, and that of the parhelium series green, the colour of the discharge is changed from yellow to green when the pressure is reduced. There is another spectrum associated with helium which is analogous to the secondary spectrum of hydrogen in that it only appears with any considerable intensity when the gas is exceedingly pure. This spectrum is known as the band spectrum of helium, and its occurrence in a gas which is known to be incapable of forming molecules, in the chemical sense of the word, is very remarkable, in view of the fact that band spectra are generally attributed to molecules. It may perhaps be suspected that there is some temporary association of atoms during the passage of the electric discharge which cannot be referred to as a molecule in the chemical sense of the word. Professor Fowler has shown that the arrangement of the heads of the bands in this spectrum resembles that found in series of lines

which are due to atoms, though the arrangement of the lines which constitute each band is of the type usually found in band spectra. When powerful condensed discharges are passed through helium a spark spectrum is developed. Two series in this spark spectrum are known as the 4686 and the ζ Puppis series, and their discovery by Professor Fowler has led to some of the most important developments of theoretical spectroscopy. These spark lines of helium are found in the nebulæ and early type stars, and are attributed to helium atoms which have lost an electron.

The energy required to produce spark spectra varies widely with the nature of the gas under investigation, and for elements of the same chemical group is, as a rule, smaller the greater the atomic weight of the element. Thus, in the case of helium powerful discharges are required for the production of the spark spectrum, and the lines of the arc series are always bright. In the case of argon a much less intense discharge is required to produce the spark lines, and with very powerful discharges the arc lines disappear almost entirely from the spectrum. In addition to the production of these spark spectra one of the effects of powerful condensed discharges is to alter the relative intensities of the arc lines. Generally speaking, the effect of an increase of energy on a particular series of lines is to enhance relatively the more refrangible members of the series, but the effect varies in degree for different series. Experiments of this kind enable us to imitate to some extent in the laboratory the distribution of intensity amongst the lines which is found in nebular and stellar spectra. It will be seen that whilst many variations in spectra can be referred to different compounds, to molecules, and to uncombined atoms in successive stages of ionisation, there are a number of other changes for which there is at present no obvious theoretical explanation. The possibility of some specific influence of one gas on the spectrum of another must now be recognised apart from the formation of chemical compounds, which, in the action of helium on the spectrum of hydrogen, appears to be excluded. There is other evidence, based on a study of the broadening of spectrum lines, of a specific action on neighbouring atoms. We are still awaiting a satisfactory theoretical explanation of phenomena of this kind, though it is now forty years since what is perhaps the first known example, the action of sodium on the absorption spectrum of magnesium vapour, was observed by Professor Liveing and Sir James Dewar in this Institution.

[T. R. M.]

WEEKLY EVENING MEETING,

Friday, March 17, 1922.

Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S., Secretary and Vice-President in the Chair.

A. P. Laurie, M.A. D.Sc., Principal, Heriot-Watt College, Professor of Chemistry to Royal Academy.

The Pigments and Mediums of the Old Masters.

[Abstract.]

Principal Laurie began by giving a short account of the special colours used in Egypt and the method of preparation, throwing on the screen photographs taken of the old Egyptian blue and of modern samples made in the Heriot-Watt College.

He then went on to describe the pigments used by the old Byzantine monks from the 7th Century onwards, with photographs taken in natural colours of the minerals from which the colours were prepared and the colours themselves, and illustrations from some of the early Byzantine manuscripts in the British Museum.

His next illustrations were taken from Scoto-Irish manuscripts, many of which are in Edinburgh, showing the close connection between the technique of the Byzantine and Irish monks.

Other illustrations were taken from the Lindisfarne Gospels produced by the monks in Holy Island.

He then went on to trace the gradual development of the illuminator's art, with illustrations from illuminated manuscripts produced by the monks of Winchester and Canterbury, and by the Flemish artists of the 15th Century.

In the course of the lecture Professor Laurie gave some account of the methods he had used for identifying pigments on manuscripts and pictures, and gave a table showing what pigments had been used from the 7th to the 17th Centuries in illuminating work and picture painting.

Turning then to painters, he illustrated the early Italian School and the work of the primitives, both in Italy and the North, by means of examples taken from the National Gallery of Edinburgh, and explained the different methods and mediums used by Italian and Northern painters.

[A. P. L.]

WEEKLY EVENING MEETING,

Friday, March 24, 1922.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

F. G. DONNAN, C.B.E. D.Sc. F.R.S. M.R.I., Professor of Chemistry, University of London.

Auxiliary International Languages.

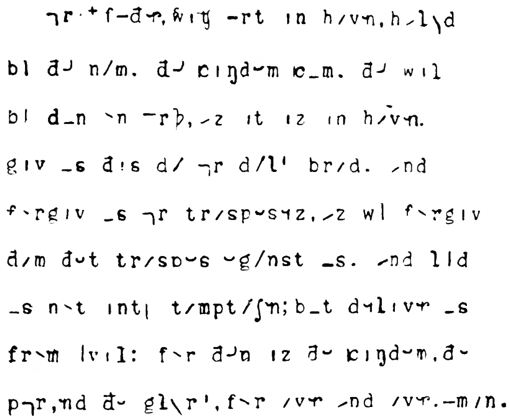
AT the present day the rights of all nations to unity, to the preservation and independent development of national life and customs, are fully recognised and admitted. Partly as a result of the war, long dormant hopes and moribund languages have awakened to a new period of life and activity. We live amidst a remarkable efflorescence of national diversity and national pride.

At the same time the material means of intercommunication by land, sea, and air are rapidly increasing in speed, efficiency and cheapness. You can lunch quietly and leisurely in Amsterdam, and the same afternoon have tea with a friend in London. Science and industry are advancing with giant strides, and in rapidly increasing measure all nations are taking part in this work. The modern world is thus a vast arena of conflict between separating and intermixing forces. In the loom of life a myriad coloured threads are intertwined in the strange fabric of modern civilisation. But where are the integrating influences that will give us that *unity in diversity* which all wise men seek? It is not a monotonous unison of thought that I mean, but a harmony of independent notes—an integration, and not a unification, of separate ideas. What is it that, whilst conserving the independent life of nations, will produce a common liberality of thought and action? There is only one answer—the intercommunication, the internationalisation of thought. Men have dreamed of a common political organisation of the world, of a human family one in government, speech and religion. Such things may, perhaps, come to be, but they lie in the shadowy realm of a very distant future. The practical problem of to-day is the problem of mutual intercomprehension, of unity of understanding, amidst variety of thought, speech and action. The solution of this problem lies in the existence of an auxiliary language common to all the

nations of the world; what we may therefore call an auxiliary international language.

As late as the 18th Century, Latin served the purpose of an auxiliary international language for the learned world, whilst French has long held sway as the common language of diplomacy (though recent events have tended to give English an equal rank). It may come to pass in the distant future that one of the great modern languages will be gradually accepted by all nations as a common auxiliary tongue known to and used by all. Many Englishmen fondly believe that this high destiny is reserved for their mother language. The very unphonetic character of English spelling presents a great difficulty in this connection. One might, however, avail oneself of the work of the Simplified Spelling Society, of which the following is an example:—"It is the jeneraishonz of children tu kum hoo apeel tu us tu saiv them from the aflikshon which we hav endeured and forgoten."

Or we might use the very scientific phonetic script of Dr. Wilfred Perrett, which he calls "Peetickay." I will show you the Lord's Prayer in English according to this phonetic manner of writing:—



With all due respect one cannot help feeling that the problem of an auxiliary international language is not to be solved in this way.

Those who have given the greatest amount of study to this subject have come to the conclusion that the world will not accept any living national language as a common medium of intercommunication. Feelings of national jealousy, prestige, and advantage are too strong. The international auxiliary language must be *neutral*. It must also be simple and regular, and simplicity and regularity are not qualities possessed by any living national language. From various points of view Latin would satisfy the condition of neutrality,

and there are some who urge the claims of this language. But apart from other obstacles the intrinsic difficulty of Latin is too great. Later on, however, I shall have something to say about the possibilities of a sort of simplified Latin.

The object of an auxiliary international language is not to displace or replace existing languages, but to protect and supplement them. These qualities of neutrality, simplicity, regularity, and compatibility can be obtained only by means of an *artificial auxiliary language.* Now this word *artificial* shocks and frightens people. We are so accustomed to the historical and analytical treatment of languages that we have never dreamt of the possibilities of synthesis. The chemists and physicists have analysed nearly all the things they have found in this world. But if they had rested content only with analysis the practical world would have much less to thank them for. We may not like synthetic butter and synthetic milk, but we have no objection to synthetic soap or synthetic glass. Why not then a synthetic language? So far as the languages of North and South America and of Western Europe are concerned, the problem is mainly one of the synthesis of existing elements, since amongst these languages there exists already a very large international vocabulary. As Dr. Cottrell has aptly expressed it, our problem is nothing less and nothing more than the science of *synthetic linguistics.* Looking at the matter from this point of view, we see that the word "artificial" is a misnomer. It is true that the first attempts to solve the problem of an auxiliary international language might be fitly termed artificial. They take us back to the 17th Century. Impressed by the logical manner in which mathematical symbolism represents complex trains of thought, in a form at once intelligible to mathematicians of all countries, some of the greatest philosophers and mathematicians of that century conceived the idea of an international language which would be a logical algebra of general thought. Descartes in 1629 discussed this idea in a letter to his friend Mersenne. Leibniz devoted many years to the problem, though he considered that for immediate practical purposes a simplified and regularised grammar applied to the word elements of Latin would provide the best solution.

Language systems of this sort are called "philosophical" or à priori. In their construction we might endeavour to make a list of all the primary ideas, and assign arbitrary written symbols, which may be also pronounceable sounds, to these. With the various permutations and combinations of these symbols we might then form all derived ideas. It is clear that from a very few symbols we can easily, by means of their permutations and combinations, form thousands of derivatives. When the number of primary ideas or elements is relatively small such systems are of great use, and are largely used. The various special codes used in international commerce are examples of this method. Another example of such an

international code language may be seen in the nomenclature and symbolism of chemistry. Thus

H_2SO_4 and para-nitro-anilin

are intelligible to chemists of every nationality. But for general purposes such systems would become exceedingly complex. Moreover, it would be very difficult to draw up a simple and fixed table of primary and fundamental ideas, for although the fundamental data of sense may remain invariable, the intellectual activity of the human mind is constantly penetrating the screen of sense-perception. Thus new concepts and ideas in accord with our progressive discovery of the real structure and activity of the world are being constantly formed.

The inventors of à priori philosophical languages have, however, usually proceeded in a somewhat different fashion, their object being to construct a vocabulary that would be based on a rational system of classification corresponding to our knowledge of things. Thus, in the 17th Century a Scotchman, George Dalgarno, and also the celebrated Bishop Wilkins—one of the founders of the Royal Society—produced two such philosophical systems. That of Bishop Wilkins was entitled "The Essay towards a Real Character and a Philosophical Language" (London, 1668). In the 18th Century the disciples of Condillac, the Ideologists, took up the problem of an artificial language considered as a classification and notation of ideas; whilst in the middle of the 19th Century the learned Spanish professor, Bonifacio Sotos Ochando, published a very perfect system of this type, in which both the grammar and the vocabulary were very fully worked out.

In his "Lectures on the Science of Language," delivered before the Royal Institution fifty-nine years ago, Max Müller discussed the possibility of an artificial language, and gave an account of the system of Bishop Wilkins. Speaking in this connection, he said:—"It is the fashion to laugh at the idea of an artificial, still more of a universal language. But if this problem were really so absurd a man like Leibniz would hardly have taken so deep an interest in its solution. That such a language should ever come into practical use, or that the whole earth should in that manner ever be of one language and one speech again, is hard to conceive. But that the problem itself admits of a solution, and of a very perfect solution, cannot be doubted."

In order to understand the method employed by Bishop Wilkins I give here the basis of his system of classification:—

	A. Transcendental Notions	Divided into 6 Genera
Five Categories of Logic	B. Substances C. Quantities D. Qualities E. Actions F. Relations	Divided into 34 Genera

These forty fundamental genera were divided into numerous species, and to all these genera and species letters of the alphabet were assigned in a regular ordinal manner. Thus the genus "element," one of the types of "substance," was denoted by De. Now Bishop Wilkins followed the peripatetic philosophy, and divided the genus element into the species earth, air, fire and water.

Substance

Element = De

Fire = Deb

Flame = Deba

De = Element   Due = elementary
Do = Stone   Duo = stony

Fire thus became Deb, and flame, a variety of fire, became Deba. Grammatical function was indicated by appropriate letters, e.g. De = element, Due = elementary, Do = stone, Duo = stony.

We can perceive here two of the fundamental objections to all such philosophical systems. In the first place all such classifications are fleeting and transient. At best they can but reflect the knowledge and science of their day. But as this is constantly changing there is no finality. We no longer accept the earth, air, fire and water of the Aristotelian-scholastic philosophy as a satisfying classification of elementary substances. Even the chemical elements of twenty-five years ago are dissolving before our eyes into the electrons, protons, and neutrons of a newer philosophy. But even were there a finality of knowledge such classificatory symbolisms would be very difficult to memorise. We should have to remember not only the symbols and their meanings, but also the whole ordinal system of assignment. In practice we should have to learn the system empirically, as we do natural living languages. Thus all the hoped-for advantages would disappear. To a child *Deba* might soon come to mean flame, but if we came across this mysterious word in later life we should have painfully to de-code it.

After this discussion it will be unnecessary to dwell further on the numerous attempts which have been made to construct such à priori philosophical languages. A glimpse at the work of Sotos Ochando must suffice. Here it is:—

A = inorganic material things

Ab = material objects

Aba = simple bodies or elements

Ababa, Ababe, Ababi, etc.
‖ ‖ ‖
Oxygen, Hydrogen, Nitrogen

A lecture in chemistry on these lines would be an interesting experience, but one not likely to be repeated.

The modern era, the era of synthetic or à posteriori as contrasted with purely à priori languages, began with Volapük. This was the discovery of Monsignor Johann Martin Schleyer, a Roman Catholic priest, of Baden in Germany, and was given to the world towards the end of the year 1880. His vocabulary consisted of root-words, derived words, and compounds. Schleyer endeavoured to borrow his root-words from the international stock, so that the greatest number of persons might have the fewest unfamiliar words to memorise. He stated himself that the Volapük Lexicon was mainly based upon the English language, because it was spoken by 100 million people. Unfortunately for the 100 million, these roots were so changed by Schleyer that a very large number of them became unrecognisable in the written language. There were several reasons for this. His system was a phonetic one, but the sounds corresponding to several of his letters were so chosen as to destroy the international appearance of the roots. No stem or root which was declinable could end in the sibilant consonants *c*, *j*, *s*, *x* and *z*, since the plural was formed by the letter *s*. Monsignor Schleyer held that the letter *r* offered such difficulty of pronunciation to children, Englishmen and Chinese—a majority of mankind—that it had to be very largely eliminated. For *r* he very often substituted the letter *l*. Finally, he made his roots as monosyllabic as possible. The net result of these transformations was that many roots chosen from English, or other languages, on account of their internationality became unrecognisable. The following examples will demonstrate this:—

Volapük Root Transformations.

Father = Fat	Rose = Lol
Knowledge = Nol	Abundance = Bundan
Speech (speak) = Pük	James Johnson = Cems Consn
World = Vol	Roof = Nuf
Chamber = Cem	Chief = Cif
Friend = Flen	

In spite of these defects Schleyer had hit on the *fundamental idea of a synthetic language compounded from internationally known roots.*

In forming derivatives he had a large number of affixes possessing more or less definite meanings, and a series of characteristic suffixes. He had thus hit on the second fundamental idea of *autonomous word formation* from the roots. Here are some examples:—

Volapük Word Derivation.

Suffix "am" = action
fom = form
fomam = formation
Suffix "it" = names of birds
Gal = evening
Galit = nightingale

Having got his vocabulary, Schleyer's object was to construct by means of a perfectly regular and uniform system of inflexions a most complete and rich grammar, one that would surpass all living languages in its variety and in its power of expressing shades of thought. All nouns had a declension, e.g. :—

Volapük Declension.

	Singular	Plural
Nominative	Dom ...	Doms
Genitive	Doma ...	Domas
Dative	Dome ...	Domes
Accusative	Domi ...	Domis

The conjugation of the verb was so rich that the Volapük verb was claimed to possess more than half a million forms. Professor Guérard quotes an amusing example of this inflexional or agglutinative wealth :—

Ulöfofs-öz

"Ladies, I charge you to have loved by a certain time!"

As Professor Guérard has well expressed it, Volapük grammar is an example of the synthetic method run riot. Volapük belongs to the class of "mixed" languages, in which borrowed and arbitrary elements are more or less logically combined. Nevertheless, in spite of its many difficulties and its à priori elements, it represented an enormous advance on the purely artificial or à priori systems of Wilkins, Sotos Ochando, and many others. It presents us with the first great attempt to build up, from a small stock of existing root-words, a synthetic auxiliary international language based on an autonomous system of word-formation and on a perfectly regular inflexional grammar. In its day it had a great success. At first it spread slowly, but about 1885 it was actively taken up in France, its chief partisan and exponent being Dr. Auguste Kerckhoffs, Professor of Modern Languages at the School of Higher Commercial Studies in Paris. From France it spread to all parts of the world. Three International Congresses were held, the third taking place in Paris in 1889. At that time there were 283 Volapük Clubs spread all over the world, 316 textbooks had appeared, and there were some 30 periodicals appearing in Volapük or dealing with it. In order that you may see a sample of this language I give here the Lord's Prayer in Volapük :—

"O Fat obas, kel binol in süls, paisaludomöz nem ola. Kömomöd monargän ola. Jenomöz vil olik, as in sül, i su tal. Bodi obsik vädeliki giovolös obes adelo. E pardolös obes debis, äs id obs aipardobs debeles obas. E no obis nindukolös in tentadi, sed aidalivolös obis de bad. Jenosöd."

Some of it looks rather strange to our eyes. But it was this very strangeness that gave Volapük a definite and distinctive character, and prevented it from looking like a hotch-potch of existing languages. The disappearance of Volapük was due largely to the internal dissen-

sions of its partisans, some of whom, led by Dr. Kerckhoffs, wished to make it simpler and more adapted to the needs of commercial life. These attempts at reform were, however, resisted by the learned originator. No doubt his system was too complicated and intricate for the majority of people. Moreover, those who took an interest in the problem of an auxiliary international language were soon provided with the much simpler and more practical Esperanto.

The author of this language, Louis Lazarus Zamenhof, was born in 1859 at Bielostok, in what was then Russian Poland. Perceiving the racial and linguistic hostilities of his native country, as a young school student in Warsaw he already dreamed of a universal neutral language and of a universal brotherhood founded thereon. He graduated as a physician at Warsaw, but during the six years of his university course he worked constantly at his secret project. At first he thought of reviving Latin, or of constructing an à priori or philosophical language. It was the study of English, however, that first showed him what could be done by means of a simple grammar, and how stems of different origins could be utilised in the construction of a harmonious and self-contained language. In 1885 his work was complete, but it was only in 1887 that he found a publisher. In that year there appeared in Warsaw a Russian pamphlet describing "La Lingvo Internacia de la Doktoro Esperanto" ("The International Language of Dr. 'Hopeful'").

In 1900 there appeared the "Universala Vortaro de la Lingvo Internacia Esperanto," by L. Zamenhof. In this dictionary the equivalents were given in five languages. The pseudonym "Esperanto," originally adopted by Dr. Zamenhof, has been transferred to the name of the language. The progress of Esperanto was at first slow. But in 1898, when the French took the lead, expansion became rapid. The Marquis Louis de Beaufront became the leader of this movement. In 1914, when the war broke out. there were over a hundred Esperanto periodicals, some appearing in Esperanto only, others in Esperanto and a national tongue. In 1905 an International Convention or Congress was held at Boulogne. Since then twelve other International Congresses have been held, the thirteenth at Prague in 1921.

As an international auxiliary language Esperanto has had an unparalleled success. It has done more to spread the idea of the need for and the possibility of an auxiliary international language than any other project.

What, then, was the nature of the discovery made by this young Polish physician, and wherein did it differ from Volapük?

Curiously enough, there was no new discovery. The fundamental ideas of Zamenhof were very largely those of Schleyer. A phonetic system, a regular method of pronunciation, a vocabulary of root-words drawn from the international treasury, an autonomous system of word-formation, and a perfectly regular grammar. In other words,

an à posteriori synthetic language. But in practice the contrast was enormous. Zamenhof did not transform and distort his international roots like Schleyer. He carried out the choice of international stems on a much broader basis. His grammar was enormously more simple and practical. The inflexional richness of the work of the learned and scholarly Schleyer disappeared, and together with it most of his à priori and arbitrary elements. Zamenhof's autonomous system of word-derivation by means of affixes of fixed and definite meanings and by means of root-combinations was immensely superior. The arbitrary characteristic endings corresponding to a classification of ideas, a relic in Volapük of the earlier à priori philosophical systems, disappeared in Zamenhof's language. The idea of using only monosyllabic roots was given up, and so the international appearance of these could be much better preserved. Here are some examples of Esperanto grammar :—

Esperanto Substantive.

Kara Amiko = Dear Friend.
Karaj Amikoj = Dear Friends.

All nouns end in *o*, all adjectives in *a*. The plural is formed by the addition of the letter *j*, and in the plural there is concord between the adjective and noun. Thus there is no grammatical gender. Of the declension of the substantive Zamenhof retained only the accusative, which is indicated by *–n*. The Esperanto conjugation of the verb is very simple, ingenious and elegant. The verb is invariable in person and number, but the tenses are indicated by inflexions. Thus :—

Esperanto Verb.

Infinitive	–i	Future	–os
Present	–as	Conditional	–us
Past	–is	Imperative Subjunctive	–u

Participles as follows :—

	Active	Passive
Present	–anta	–ata
Past	–inta	–ita
Future	–onta	–ota

In the conjugation both active and passive tenses are expressed by means of one auxiliary, esti = to be. This may be seen from the following excerpt :—

Esperanto Verb.

Ami = to love.

Me estas amanta = I am loving
Mi estas amata = I am loved
Mi estas aminta = I have loved
Mi estas amonta = I am going to love
Mi estis aminta = I had loved
Mi estas amita = I have been loved
Mi estos aminta = I shall have loved
Mi estos amata = I shall be loved
Mi estos amita = I shall have been loved

Esperanto is very proud of its celebrated table of correlative words. Although this table is very ingenious and neat, it represents an arbitary à priori element in the Esperanto language. Here is an example :—

Indef.	Distrib. Collective	Interrog. Relative	Negative	Demonstr.
Time : Iam	*Ĉiam*	*Kiam*	*Neniam*	*Tiam*
At some time. At any time. Ever.	All the time. Always.	At what time? When?	At no time. Never.	At that time. Then.
Place : Ie	*Ĉie*	*Kie*	*Nenie*	*Tie*
Somewhere. Anywhere.	In every place. Everywhere.	In what place? Where?	In no place. Nowhere.	In that place. There.

To see the great practical progress made by Zamenhof as compared with Schleyer it is sufficient to compare the following version of the Lord's Prayer in Esperanto with the Volapük example previously given :—

"Patro nia, kiu estas in la ĉielo, sankta estu via nomo; venu regeco via; estu volo via, tiel en la ĉielo, tiel ankaŭ sur la tero. Panon nian ĉiutagan donu al ni hodiaŭ; kaj pardonu al ni ŝuldojn niajn, kiel ni ankaŭ pardonas al niaj ŝuldantoj; kaj ne konduku nin en la tenton, sed liberigu nin de la malbono."

Here is another sample of Esperanto, culled from the Esperanto Manual of Margaret L. Jones :—

"Unu el la ĉefaj taskoj de ĉiu homo estas la perfektigo de si mem. Ĉiu homo devas konsideri ke li estas ĉiama lernanto, ĉiama studento, ĉiama edukato. Ĉiutage li devas demandi sin, ĉu, tiun tagon, li estas pli perfekta ol la antaŭan; ĉu li faris progreson en la perfektigado de sia korpo kaj de sia spirito."

In spite of many obvious and indeed glaring defects, Esperanto is undoubtedly, so far as numbers are concerned, the greatest and most successful linguistic experiment that the world has yet seen. Let us not criticise too severely the work of a man who was neither a great scholar nor a great professional philologist, but let us rather admire the splendid effort which he made. His work has been of the greatest service in demonstrating to an indifferent world the practical possibility of an auxiliary international language. So great was the interest taken in this branch of science at the Paris Exhibition of 1900 that, under the leadership of M. Leau, a French professor of mathematics, a number of scientists and delegates from learned societies were gathered together, and on January 17th, 1901,

the "*Delegation for the Adoption of an Auxiliary Language*" was founded. After a great deal of preliminary work on the subject, the matter was submitted, through the kind offices of the Imperial Academy of Sciences at Vienna, to the International Association of Academies, which on May 29th, 1907, declared itself incompetent to deal with the question. The Delegation then proceeded itself to elect a special Committee to study the problem. This Committee embraced a number of distinguished authorities on science and linguistics, and included the two secretaries, Professors Couturat and Leau. After eighteen sittings, held at the Collége de France, the following decision was arrived at:—

"None of the proposed languages can be adopted *in toto* and without modification. The Committee have decided to adopt in principle Esperanto, on account of its relative perfection and of the many and varied applications which have been made of it; *provided* that certain modifications be executed by the Permanent Commission, on the lines indicated by the conclusion of the Report of the Secretaries and by the project of Ido, if possible in agreement with the Esperantist Linguistic Committee."

It appeared later that the "project of Ido" was an anonymous pamphlet proposing a number of reforms in Esperanto, whose real author was the Marquis de Beaufront, up to that time the most eminent supporter of Esperanto in the world. Messieurs Couturat and Leau had made a most exhaustive and scholarly study of all known auxiliary languages, their labours being embodied in a masterly book entitled "Histoire de la Langue Universelle," and also in another one entitled "Les Nouvelles Langues Internationales." Their Report to the Committee indicated very clearly the lines along which Esperanto could be improved. As the Esperanto Linguistic Committee declined to collaborate, the Committee of the Delegation appointed a Permanent Commission to carry out the reforms which they had in view, and as they were unable to use the name Esperanto the reformed Esperanto was called *Ido*. Thus was born the

Linguo Internaciona Ido.

The principal part in this work was taken by Couturat, who was well-known for his studies in logic and his work on Leibniz. The changes effected dealt principally with the following points:—

1. The elimination of Zamenhof's accented letters, and the choice of a phonetic alphabet which would be better adapted to the international recognition of the root-words.

2. The elimination as far as possible of the arbitrary à priori elements in Esperanto.

3. The application of a definite and systematic method in the choosing of the internationally existing roots.

4. The carrying out of a more logical method of word-derivation

from the roots by means of the derivative affixes, and a better choice of the latter.

5. Certain changes in the grammar, such as the dropping of the accusative inflexion, except where it is required to prevent ambiguity; the dropping of the inflexional plural for adjectives; certain changes in the verb such as the infinitive in –ar, instead of –i, and the introduction of a synthetic passive form; the plural of nouns formed by –i, instead of –oj: etc.

In its basic ideas Ido is a language of the same type as Esperanto. It is a great pity that all parties could not have combined at an early stage in the development of Ido. I shall not, however, enter into any details or express any opinion here concerning the very bitter mutual hostility which arose. It was very human, and one can understand the reasons. After all, the stability of society depends on the existence of Conservatives as well as Liberals. Perhaps in this instance the worthy Conservatives mistook the good Liberals for anarchists. That is not an unknown phenomenon in very exalted circles.

If I may be allowed a personal opinion, I will say that most, if not all, of the Ido improvements appeal to me very strongly. If we are to choose a language of the Esperanto type, and if the choice lies only between Esperanto and Ido, I would choose Ido. I do not say this for any propagandist purposes, and I say it with a full appreciation of the splendid early work of Dr. Zamenhof. But at the same time, I have an equally great admiration for the splendid later work of Professor Couturat and his collaborators.

As you will wish to see some samples of Ido here is the Lord's Prayer in that language:—

"Patro nia, qua esas en la cielo, tua nomo santigesez; tua regno advenez; tua volo facesez, quale en la cielo, tale anke sur la tero. Donez a ni cadie l'omnidiala pano; e pardonez a ni nia ofensi, quale anke ni pardonas a nia ofensanti; e ne duktez ni aden la tento, ma liberigez ni del malajo. Nam tua esas la regno, la povo e la glorio eterne. Amen."

Here is another specimen, translated from "The Laws of Habit," of Professor W. James.

La Legi dil Kustumo.

"Me kredas ke ni esas submisata a la lego dil kustumo per konsequo dil fakto ke ni havas korpi. La plastikeso di la vivanta materyo di nia nerva sistemo esas, abreje, la kauzo ke ni facas un kozo malfacile la unesma foyo, sed balde plu e plu facile, e fine, kun suficanta pratiko, ni facas ol mimekanike, e kun preske nula koncio."

Professor Couturat has done a great deal of fine work in conferring on Ido a very logical system of autonomous word-formation from the root-words by means of a well-chosen system of derivative

affixes. His "*Étude sur la Dérivation*" is an extremely interesting application of logic to the science of synthetic linguistics.

Here are some examples as applied to Ido :—

Ido Word Derivation.

Stem		
Kron	Kron–o	= a crown
	Kron–iz–ar	= to crown
	Kron–iz–o	= coronation
	Kron–iz–ad–o	= continued action of crowning
Bel	Bel–a	= beautiful
	Bel–o	= beautiful person
	Bel–es–o	= beauty
	Bel–ig–ar	= to beautify
	Bel–ig–o	= beautifying
Joy	Joy–o	= joy
	Joy–oz–a	= joyous

These examples illustrate the following principles :—

1. There must exist a unique and reciprocal correspondence between the ideas and the word elements which express them.

2. Every word element represents an elementary idea, which is always the same, so that a combination of elements has a meaning determined by the combination of the corresponding ideas.

3. Every derivative must be *reversible*—that is to say, if one passes from one word to another of the same family in virtue of a certain rule, one must be able to pass inversely from the second to the first in virtue of a rule which is exactly the inverse of the preceding (principle of reversibility).

It will be seen that these principles are simply the rules of mathematical logic as applied to the operations of word-derivation. But only time can show whether the strict principles of symbolic logic will appeal to the mind of a child or to that of a comparatively unlearned person. Their value has been contested by De Saussure. Certainly neither Esperanto nor any living national language conforms strictly, or even approximately, to such mathematical precision and regularity. Languages have grown up in the rough and tumble of daily life, and are consequently capricious and irregular. That is of course no reason why a synthetic auxiliary language should purposely be made illogical or irregular. But we must be careful to remember that the learning of a language is a question of the psychology of the child mind. There may be a playful little imp here that is not impressed by the mathematical precision of polished logicians. Perhaps we must not get *too* far away from "did ums" and "tootsy-wootsy."

Ido, like Esperanto, has had a very great success, and has been very thoroughly developed. Many general and technical dictionaries

have been worked out. Before the war there appeared ten or twelve periodicals dealing with or written in this language. The International Ido Academy has done very fine work in bringing it to as high a state of perfection as possible. Very many Ido Clubs and Societies have been formed in all parts of the world, and already a very considerable literature exists. We may say that the Ido, like the Esperanto, movement has done immense service in familiarising the world with the practicability of an international auxiliary language. Both these great linguistic experiments are of profound interest and importance.

I must now lead your thoughts away from Esperanto and Ido, and back to the International Academy for a Universal Language, which was founded by the two International Volapük Congresses of 1887 and 1889. This Academy continued to exist, and set itself to the task of reforming Volapük. Very important and scholarly work was done by Mr. Rosenberger, a Russian engineer, and his collaborators (Rosenberger was a Director of the Academy from 1893 to 1898). They produced a vocabulary of root-words based on the principle of maximum internationality. The greater part of these roots are common to at least four of the seven chief languages—German, English, French, Italian, Russian, Spanish and Latin. Largely as a consequence of the inclusion of Latin, the result was an almost exclusively Neo-Latin vocabulary—one much more Romanic than that of Esperanto.

A very simple grammar and a regular system of word-derivation by means of derivative affixes were introduced. But autonomous word-formation was not allowed to exclude international derivatives. There was no constant ending to distinguish, as in Esperanto, a noun, an adjective, or the present indicative of a verb. Natural gender was indicated by final *–o* and *–a*: when necessary, the plural by *–i*. The adjective remained invariable, unless it was used as a substantive. There was no article, definite or indefinite. The conjugation of the verb was as follows:—

Verb Conjugation.

Amar = to love

Mi am = I love
Mi amav = I loved
Mi av amed = I have loved
Mi avav amed = I had loved
Mi amero = I shall love
Mi avero amed = I shall have loved
Mi amerio = I would love

Participles: amant, amed.

Passive by means of the verb esar (to be) and the past participle amed.

Thus was produced about 1903 the language

IDIOM NEUTRAL,

the descendant of Volapük, though scarcely any trace of the parental features remained. The fundamental principles of this language are unassailable. Its choice of international stems, so far as it has gone, can scarcely be improved. But in its desire to be as international as possible, and at the same time to retain a regular system of word-formation, it was torn between the two conflicting ideas of :—

(1) The adoption of international derived forms as well as roots.

(2) The perfectly regular and autonomous formation of words by means of invariable though international affixes.

Also, its phonetic system concealed the international form of many words. Here is the Lord's Prayer in the 1903 form of Idiom Neutral:

"Nostr patr kel es in sieli! Ke votr nom es sanktifiked; ke votr regnia veni; ke votr volu es fasied, kuale in siel, tale et su ter. Donu sidiurne a noi nostr pan omnidiurnik; e pardona a noi nostr debti, kuale et noi pardon a nostr debtatori; e no induka noi in tentasion, ma librifika noi de it mal."

But Mr. Rosenberger did not rest satisfied with this. In 1907–8, in collaboration with Mr. De Wahl, he introduced various reforms. The spelling was rendered more in conformity with international graphism. The language was rendered more naturally Neo-Latinic, more à posteriori, but at the sacrifice of regularity. Here is the Lord's Prayer in reformed Idiom Neutral of 1907 :—

"Nostr patr, qui es in cieli! Que votre nom es sanctificat; que votr regnia veni; que votr voluntat es facit, quale in ciel, tale anque su terr. Dona nos hodie nostr pan quotidian; e pardona nos nostr debti quale anque nos pardona nostr debenti; e non induca nos in tentasion, ma librifica nos da it mal."

Idiom Neutral has not achieved the practical success of Esperanto and Ido. This may be because it came too late. It appeals to educated people more than Esperanto and Ido on account of its more homogeneous vocabulary, which is practically exclusively Romanic. But it has not been so fully developed as Esperanto and Ido. As a separate and independent project it may be said to have disappeared with the death of Mr. Rosenberger in 1918.

A language of the Neo-Latin type, somewhat similar to Neutral Idiom, is the

PANROMAN (or UNIVERSAL)

of the German Positivist and Pacifist, Dr. H. Molenaar. Here is the Lord's Prayer in this language :—

"Nor patr qui es in ziel; ton nom ese sanktifiket. Ton regn vene. Ton voluntat ese faket in ziel kom in ter. Done nos hodi nor pan quotidian. Pardone nos nor debeti, kom nos pardon nor debetori. E non induke nos in tentazion, ma libere nos de mal."

I must now carry you a step further in the direction in which Idiom Neutral and Panroman have been leading us. Various attempts, such as those of Mr. Henderson and of Dr. Ross, have been made to introduce a sort of simplified Latin. But the man who has defined most clearly the Neo-Latin principle, and who has not only worked the hardest in this field, but also grouped and organised many isolated workers of kindred views and affinities, is Dr. Giuseppe Peano, Professor of Mathematics in the University of Turin. In 1908 he became Director of the International Language Academy. In the "Discussiones" of that body he has published from year to year the work of himself and many collaborators. A very large amount of scholarly work has been done in the discovery of the international vocabulary common to Latin, Italian, French, English and German. The result of this etymological study may be seen in Professor Peano's important "Vocabulario Commune," the second edition of which appeared in 1915. Following the indication given by Leibniz, Peano has built on an exclusively Neo-Latin basis so far as the main vocabulary is concerned, though modern words acquiring international usage may be accepted. Partly as the result of Leibniz's views, and partly on the basis of his own reasoning, he has eliminated from grammar formal gender, declension, number, and even conjugation of the verb. Not only has he done that, but he has set his face against the system of autonomous word-formation. He holds that "either there already exists an international word or the idea can be expressed by a combination of international words." The result is his

LATINO SINE FLEXIONE,

or flexionless Latin, which might perhaps be also described as the Christmas Eve dream of the English school-boy, or the terrible nightmare of the Classical Sixth Form Master. Here is the Lord's Prayer in this novel tongue :—

"Patre nostro, qui es in celos, que tuo nomine fi sanctificato. Que tuo regno adveni; que tua voluntate es facta sicut in celo et in terra. Da hodie ad nos nostro pane quotidiano. Et remitte ad nos nostros debitos, sicut et nos remitte ad nostros debitores. Et non induce nos in tentatione, sed libera nos ab malo. Amen."

You can see how it compares with the Latin of the Vulgate :—

"Pater noster, qui es in cœlis; sanctificetur nomen tuum; adveniat regnum tuum. Fiat voluntas tua, sicut in cœlo, et in terra. Panem nostrum (supersubstantialem) da nobis hodie. Et dimitte nobis debita nostra, sicut et nos dimittimus debitoribus nostris; et ne nos inducas in tentationem. Sed libera nos a malo. Amen."

Professor Peano adopts as the form of the stem the Latin accusative minus its inflexion, the result being generally the ablative. In this way he secures a harmonious language somewhat similar in

sound to Italian. For the stem of the verb he deducts *-re* from the infinitive, obtaining thus the imperative form.

It will be seen, therefore, that his nouns and adjectives contain relics of the vanished Latin declensions. But for those who are ignorant of Latin, and who do not possess in their own language a vocabulary of Latin origin, Peano gives rules whereby such unfortunates may utilise a Latin dictionary in their own language for the purpose of finding out his stems, i.e. the ablative form. For many scientific purposes Peano's flexionless Latin is ready for use. He has himself employed it for many years in his own journal, "The Mathematical Review." Here is an example taken at random from that journal :—

(G. Peano. Revista de Mathematica.)

De Infinito in Mathematica.

Per Philip E. B. Jourdain.

"Dum mathematico conceptiones et operationes es applicabile ad omni finito classe, et ad aliquo infinito classes, que pote es vocato 'transfinito,' tamen existe innumero classes ad que ce conceptus non est applicabile. Primo exemplo que occurre es minimo clase tale que omni finito aut transfinito classe pote es ordinato in simile modo ut segmento de illo."

Here is another sample of the language :—

Historia de Interlingua.

"Plure Philosopho stude lingue rationale. Descartes (1596–1650) describe uno systema. Leibniz (1646–1716) in numeroso scripto, expose ideas profundo et de vivo interesse super isto problema. In illo tempore Latino es de usu internationale. Philosophos non tracta de lingua regulare respondente ad philosophia. Es scientia simile ad Logica-mathematica, que hodie habe numeroso culture."

This sort of thing may shock the refined Classicist, but many scientific people prefer it to Russian, Basque, Magyar, Finnish, Turkish and Chinese. Peano's "Latino" may not be ready yet for the exigencies of daily life. We cannot perhaps as yet deal with the muffin-man by means of this subtle instrument of thought. Moreover, Peano's symbolic logic may have eliminated grammar more drastically than is possible in an imperfect and non-mathematical world. But it is certain that no student and investigator of synthetic linguistics can afford to neglect his work. His method is such that every Anglo-Latin stem is accepted in his vocabulary. I must confess that the very homogeneous result thus obtained is at first sight much more attractive than the pot-pourri of Esperanto and Ido. But one must not forget that this sort of "naturalness" is gained at the expense of regularity and autonomy. Latino is certainly easier and more pleasant to read, but it might be more difficult to write and speak. Time and experiment alone can settle

these questions. The true solution of the problem may consist in selecting the most international roots according to the fashion of Peano, but also the most international affixes of derivation. With these natural elements derivatives and compounds will then be formed according to simple and invariable rules. Thus the advantages of the Neo-Latin or Anglo-Latin vocabulary of stems will be combined with the regular and autonomous word-derivation of Ido. This is the view held by Professor Guérard, who has just published a most valuable book, entitled "A Short History of the International Language Movement" (Fisher Unwin, 1922).

As Professor Guérard points out, these two sets of fundamental ideas are embodied in the language project of M. Albert Michaux, entitled

Romanal.

Here, for example, is the Lord's Prayer in Romanal:—

"Patro nostri, que es in cieles, sanctificat estas nomine tui; advenias regne tui; fias volite tui, sicut en ciele, et in terre. Il pane nostri quotidiani das ad nos hodie; et dimittas nostri debites, sicut et nos dimitta debitantos nostri; et ne nos inducas en tentatione, sed liberas nos ex male. Amen."

Needless to say, Romanal is not the last word on the subject, nor is it free from debatable points. But it represents the combination of an "etymological Anglo-Latin root" vocabulary with regularity of word-derivation and simplicity of grammar.

In the preceding discussion I have endeavoured to give you a very brief account of some of the principal efforts to solve the problem. There have been very many others, and you will find a learned account of these in the works of Professors Couturat and Leau, referred to previously. The large amount of research work already done and the practical success of Esperanto and Ido prove that the problem is not an insoluble one. At first one might be inclined to think that the production of an international auxiliary language is a sort of parlour game, or at best a pure matter of caprice. Attentive study of the problem shows that this is quite a false view. Whatever may be the final solution, it is already clear that some of the fundamental principles have been elucidated. There *does* exist a science of synthetic linguistics compounded of logic, psychology and philology. It has been argued that the field hitherto traversed, at all events in the later systems, is too narrow; that the so-called international vocabularies are not really international, and apply at best only to two groups of existing languages. What comfort, it is argued, can a word such as "amico" bring to the Basques, Finns, Hungarians, Turks, Japanese, Chinese, etc.? What special comfort, I would then ask, does the learning of English,

French, German, Italian, Spanish, Dutch, Swedish and Russian bring to a young Japanese gentleman? Are we then to go back to Sotos Ochando and bring comfort to nobody? I think not. But the objection is not one to be lightly passed over. It may be that the world will require more than one auxiliary language. Two such, or even three, would be better than the necessity of having to learn a hundred living languages. Only time and prolonged study and investigation can settle questions of this order. The whole civilised world must collaborate in this investigation. There is plenty of time. We have been using an alphabet for, say, eight or ten thousand years at most, and as this planet is reckoned to be over a thousand million years old it will, barring accidents, continue to be habitable for some considerable time.

Meanwhile the problem is a very pressing one. Those who have to do with science, industry and commerce feel this very acutely. Before the war I attended several international scientific congresses. On these occasions it was open to anyone to speak in English, French, German or Italian. When the language of the speaker or lecturer changed, one half of the audience usually adjourned to the refreshment bar. I could follow German, but when it was a case of Italian or Parisian French I also used to get thirsty. I am going to an international scientific congress in June of this year. The representatives of at least thirteen different nations will be present, and I expect at least four languages will be used. As the language of the country where the congress is to be held is not one of these one ought really to know five languages. I am glad to say that the civilised world is at last beginning to take a real interest in this problem. We may, indeed, say that since the war the whole question has entered on a new phase. Learned and scientific bodies of international influence and repute are beginning to study the matter seriously. The present organised movement in this direction may be considered as dating from the adoption by the International Research Council at their meeting in Brussels in July, 1919, of the following resolutions:—

(*a*) That the International Research Council appoint a Committee to investigate and report to it the present status and possible outlook of the general problem of an international auxiliary language.

(*b*) That the Committee be authorised to co-operate in its studies with other organisations engaged in the same work, provided that nothing in these resolutions shall be interpreted as giving the Committee any authority to commit the Council to adhesion to or approval of any particular project.

This Committee is now at work. Its Chairman is Dr. F. G. Cottrell, and its headquarters are at the offices of the National Research Council of the United States, 1701 Massachusetts Avenue, Washington, D.C. This Central Committee has already done an immense amount of work in securing the organisation of committees

and working groups in the national academic organisations and educational institutions, and in co-ordinating this work and serving as a clearing-house for the exchange and distribution of information and plans. The first national response to the appointment of the International Committee was by the British Association for the Advancement of Science, which, at its Bournemouth Meeting in September, 1919, appointed a Committee "to study the practicability of an international language." This British Committee has been very active, and at the Edinburgh Meeting of the British Association in September, 1921, presented its Report. Its conclusions may be summarised very briefly as follows :—

1. Latin is too difficult to serve as an international auxiliary language.

2. The adoption of any modern national language would confer undue advantages and excite jealousy.

3. Therefore an invented language is best. Esperanto and Ido are suitable ; but the Committee is not prepared to decide between them.

The Committee is continuing to study the problem. The American Association for the Advancement of Science appointed a Committee in April, 1921, and this Committee has presented a Report which was accepted by the Council of the Association at Toronto on December 29th, 1921. The Committee recommended that the American Association for the Advancement of Science—

(*a*) Recognises the need and timeliness of fundamental research on the scientific principles which must underlie the formation, standardisation and introduction of an international auxiliary language, and recommends to its members and affiliated societies that they give serious consideration to the general aspects of this problem, as well as direct technical study and help in their own special fields wherever possible.

(*b*) Looks with approval upon the attempt now being made by the National Research Council and the American Council of Learned Societies to focus upon this subject the efforts of those scholars in this country best fitted for the task, and to transmit the results to the appropriate international bodies.

(*c*) Endorses the heretofore relatively neglected problem of an international auxiliary language as one deserving of support and encouragement.

(*d*) Continues its Committee on International Auxiliary Languages, charging it with the furtherance of the objects above enumerated, and reporting progress made to the Association at its next meeting.

The American Council on Education, the American Classical League, the American Philological Association and the National Research Council of America have also appointed Committees. Furthermore, the American Council of Learned Societies has autho-

rised the appointment of delegates to confer with the Committee of the National Research Council. Thus the national American representatives of science and of the humanities are uniting to study the problem.

Both the French and the Italian Associations for the Advancement of Science have appointed Committees to examine and report on the international language question.

On September 13th, 1921, the following resolution was presented to the Assembly of the League of Nations by delegates representing twelve States :—

"The League of Nations is well aware of the language difficulties that prevent a direct intercourse between the peoples, and of the urgent need of finding some practical means to remove this obstacle and help the good understanding of nations.

"Follows with interest the experiments of official teaching of the international language Esperanto in the public schools of some members of the League.

"Hopes to see that teaching made more general in the whole world, so that the children of all countries may know at least two languages : their mother-tongue and an easy means of international communication.

"Asks the Secretary-General to prepare for the next Assembly a report on the results reached in this respect."

With regard to this motion, the special Committee dealing with the inclusion upon the Agenda of Motions submitted to the Assembly reported to that body on September 15th, 1921, as follows :—

"The above-mentioned delegates have proposed the introduction of Esperanto as an auxiliary international language into public schools in order to facilitate direct intercourse between all nations throughout the world.

"The Committee are of opinion that this question, in which an ever-increasing number of great States are interested, should be attentively studied before it can be dealt with by the Assembly."

As a result of this the Secretariat of the League have been instructed to investigate the experiments already made, and ascertain the actual results attained.

On November 20th, 1919, some Swedish gentlemen interested in the question of an international language formed a Committee to promote this subject and to unite the various interests concerned. This Committee has brought the matter before the Swedish Parliament, and has also addressed a request to the League of Nations.

From all this it will be evident that the existence of the problem, and the urgent necessity for its study and investigation, are now fully admitted and recognised by the learned, scientific and political organisations of the highest national and international status. Before definite action can be taken by national governments there must be, however, another period of prolonged and exhaustive research and

experiment. This work must be, as we have every reason now to hope and expect, internationally co-ordinated and supported. Those who have laboured manfully in the past, and the many who have given their adherence to this or that special solution, must be prepared to co-operate without bias and without sorrow. The subordination of self and of the most dearly held, the most beloved possessions of the mind in the interest of intellectual advance and the common good of humanity is the spirit of true science.

[F. G. D.]

WEEKLY EVENING MEETING,

Friday, March 31, 1922.

Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S., Secretary and Vice-President, in the Chair.

Arthur B. Walkley, Author of "Drama and Life," etc.

Jane Austen.

[Lecture in full appeared in the "Nineteenth Century" for April 1922.]

GENERAL MONTHLY MEETING,

Monday, April 3, 1922.

SIR JAMES REID, G.C.V.O. K.C.B. M.D. LL.D. F.R.C.P., Vice-President, in the Chair.

Arthur James Bryant,
Robert Henry Cole, M.D.
Jacob Lerman,
Frederick William Stevenson,

were elected Members.

The Chairman announced the decease on March 27, 1922, of Professor P. A. Guye, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

The Managers of the Royal Institution of Great Britain desire to place on record their sense of the great loss the Royal Institution has sustained by the death of Philippe Auguste Guye, D.Sc. (Geneva and Paris), Professor of Chemistry in the University of Geneva, Corresponding Member of the Institute of France, Honorary Fellow of the Chemical Society of London, an Honorary Member of the Royal Institution.

Professor Guye enriched the Science of Physical Chemistry by his masterly researches on Stereochemistry; Gaseous Density data in Organic and Inorganic Chemistry; and the production of Synthetic Nitric Acid: recorded in numerous Memoirs contributed to the Transactions of Learned Societies. He was the Founder of the "Journal de Chimie Physique," which he edited from 1903 to 1922, and in which he published many of his Scientific Papers.

The Managers desire to express on behalf of the Members their sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Lisbon, University of—Arquivo de Anatomia, Vol. V. Nos. 1–3; Vol. VI. No. 1; Vol. VII. No. 1. 8vo. 1921.
London County Council—Gazette, March 1922. 4to.
London University—Gazette, March 1922. 4to.
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WEEKLY EVENING MEETING,

Friday, April 28, 1922.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

ARTHUR HARDEN, D.Sc. F.R.S.

Vitamin Problems.

[ABSTRACT.]

THE existence of three vitamins, termed A, B, and C, has now been firmly established, and a general idea has been obtained of their distribution among animal and vegetable organisms. Hitherto, comparatively little quantitative work has been done in this direction, and further progress must depend on a more general adoption of quantitative methods. These are at present tedious and not very accurate. In the case of each of the vitamins the requirements of the special animal employed serve as the unit of comparison, and these vary considerably from individual to individual, so that many observations are necessary if any, even moderate, degree of accuracy is to be attained. Thus in the estimation of the antiscorbutic potency of food materials, by the method worked out by Miss Chick and her colleagues at the Lister Institute, it has seldom been possible to achieve a greater accuracy than about 25–50 per cent. This obviously imposes a very serious limitation on any attempts to study variations in potency unless these are of a very gross order. Another great difficulty inherent in this kind of observation is that when the potency is low, the necessary dose of the material to be tested is correspondingly high, and soon transcends what is permissible without interference with other necessary conditions of the diet, such as protein content, etc. Very much the same conditions hold with regard to Vitamin B, especially when this is estimated by the effect of the material on the growth of rats; and, as a matter of fact, the great bulk of the work carried out in America by this method is not strictly quantitative, but simply leads to the result that a certain ration does, or does not, suffice for the growth of a young rat.

As regards Vitamin A, the method of Zilva and Miura promises to yield moderately accurate and consistent results. This is attained by keeping the experimental animals (young rats) on a diet totally deficient in Vitamin A until they have ceased to grow, and then

ascertaining the minimum dose of the material to be tested which will induce definite and steady growth for four weeks. Animals which do not cease to grow in three weeks are rejected, greater uniformity in the results being thus attained. The test material is, whenever possible, administered quantitatively to the animal, and not, as was formerly the practice, mixed with the ration in a known proportion. One of the immediate results of the application of this method has been the discovery that cod-liver oil, formerly classed with butter as a good source of Vitamin A, is in reality 200–250 times as potent as butter, and is, along with similar fish-liver oils, by far the richest in this material of all the substances which have so far been examined.

A further piece of information, which is essential for the detailed study of these substances, is their behaviour towards heat, oxidation, etc. In this respect some progress has been made, and it may be stated with some confidence that both Vitamins A and C are moderately stable towards rise of temperature, provided that air be excluded, whereas in the presence of air they are rapidly inactivated. Whether the effect of air is reversible or not has not yet been ascertained. Vitamin B, on the other hand, appears not to be affected by air, and is also moderately stable towards rise of temperature. None of the three vitamins is easily inactivated by hydrolysis under anaerobic conditions, and this fact has led to the interesting observation that Vitamin A, although usually associated, in the animal organism, with fat, is not itself a fat, but remains in the unsaponifiable residue with almost unabated potency. This indicates how small a weight of the vitamin itself is necessary for the daily ration of a young rat. In some cases as little as 1·2 milligram of the oil is sufficient to permit of definite growth, and of this only 1–2 per cent. is unsaponifiable, while, as is well known, the chief constituent of the unsaponifiable matter is cholesterol, which has itself no vitaminic potency. The actual requirement of the vitamin itself must therefore be of the order of 1/500 milligram per diem. The other two vitamins have not been obtained in so concentrated a form, but it appears highly probable that they too are present in foodstuffs only in infinitesimal amounts.

The origin of all three vitamins is to be sought in the vegetable kingdom. The production of Vitamin A has been followed (Coward and Drummond) from the seed, and it has been found that it does not appear until the photosynthetic processes begin. Thus sunflower seeds are almost devoid of it, and so are the etiolated seedlings formed when these seeds germinate in the dark. In the light, on the other hand, the green seedlings, grown in a medium free from the vitamin, produce it freely. This vitamin is often closely associated with the carotene and xanthophyll of plants; so intimately indeed, that it was at one time thought that it might be closely related to, if not identical with, one of them. The association, however, although very frequent,

is not essential, and no definite relation can be shown to exist between the two. Vitamin C is either absent from seeds or only present in them in very minute amount, but appears when the seed germinates and before any green parts are formed. Nothing is, however, known of the inactive pro-vitamin or of the process by which it is rendered active.

Concerning the origin of Vitamin B a considerable amount of discussion has taken place. Its presence in a large proportion in yeast points to the probability that it can be produced without the intervention of light, and both in America and in this country it has been found that yeast can actually produce the vitamin when grown in a "synthetic medium" comprising only substances of known composition and free from the vitamin in question. Recently, however, Eijkman, in Holland, has obtained a contrary result, so that at the moment this remains an open question.

The animal organism appears to be unable, in normal circumstances, to produce any of these principles for itself, and hence the amounts found in animal products depend ultimately on the diet of the animal. This opens up, among many other problems, the important question of the vitaminic properties of milk, and there seems to be no doubt, from experimental work, both here and in America, that these properties are profoundly affected by the diet of the cow. Milk obtained in winter when the animals are stall-fed has been shown to be markedly deficient in Vitamin A, and there is also great danger of a deficiency of Vitamin C. One of the pressing requirements of the moment is the careful quantitative examination of foodstuffs available for the feeding of cattle, so that a rational system of winter feeding can be adopted which will produce milk as good as that given in summer. Such an examination would seem naturally to fall within the purview of the Board of Agriculture.

The evil results of a deficiency of Vitamins B and C, especially in the diet of children, are well known—beri-beri and scurvy, latent or patent—but the effect of a lack of Vitamin A is not so well recognised or so universally acknowledged. One school considers that a deficiency of this vitamin is at least a prominent factor in the causation, if not, as they formerly held, the sole cause of rickets. Others consider rickets to be a disease brought on by non-hygienic surroundings, lack of fresh air and exercise, etc. The latest experimental results show that rickets (in rats) can be produced infallibly by dietetic changes, but that the lack of Vitamin A does not of itself lead to the disease unless at the same time the diet is faulty as regards the supply of calcium or phosphorus. This faulty mineral supply does not usually lead to true rickets if sufficient Vitamin A be present, although the bone formation under these circumstances is not quite normal. This explains the well-known curative effect of cod-liver oil in rickets. So marked is the effect of this remedy, that McCollum, not appreciating the relatively enormous concentration of Vitamin A

present in it compared with that in butter, as proved by Zilva, has suggested that cod-liver oil contains some other specific substance absent from butter, to which its great superiority is due. The difference, however, seems to be merely quantitative, and the further complication suggested by McCollum appears to be unnecessary.

These experiments on rickets have led to what promises to be a discovery of far-reaching importance. Rats on a diet, which in the laboratory will infallibly produce rickets, do not acquire the disease if they are exposed to sunlight in the open air or to ultra-violet radiation, and rats which have acquired the disease can be cured by either of these treatments, just as they can be cured by the administration of cod-liver oil. Sunlight and ultra-violet radiation have also been found to be effective cures or preventives of rickets in children. The cures by light and by cod-liver oil seem to proceed in precisely the same way, and the idea naturally suggests itself, especially to the mind of a chemist, that the light actually brings about the synthesis of the vitamin in the animal body just as it does in the plant. This idea still awaits experimental verification or disproof; but there is no doubt that this function of light will lead to profoundly important developments in our knowledge.

[A. H.]

ANNUAL MEETING,

Monday, May 1, 1922.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

THE Annual Report of the Committee of Visitors for the year 1921, testifying to the continued prosperity and efficient management of the Institution, was read and adopted.

Fifty-seven new Members were elected in 1921.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1921.

The Books and Pamphlets presented in 1920 amounted to 210 volumes, making, with 441 volumes (including Periodicals bound)

purchased by the Managers, a total of 651 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :

President—The Duke of Northumberland, M.V.O. C.B.E.
Treasurer—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
Secretary—Colonel Edmond H. Grove-Hills, C.M.G. D.Sc. F.R.S.

Managers.

Sidney G. Brown, F.R.S. M.I.E.E.
John Mitchell Bruce, C.V.O. M.D. LL.D.
Alexander H. Goschen.
Sir Alexander C. Mackenzie, Mus.Doc. D.C.L. LL.D.
Sir Ernest Moon, K.C.B. K.C. LL.B.
The Hon. Sir Charles Parsons, K.C.B. J.P. LL.D. F.R.S.
Alfred W. Porter, D.Sc. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
The Marquess of Salisbury, K.G. P.C. G.C.V.O.
Sir David Salomons, Bart., D.L. J.P. F.R.A.S.
Joseph Shaw, K.C. M.A.
William Stone, M.A. F.L.S.
Sir J. J. Thomson, O.M. LL.D. D.Sc. F.R.S.
Sir Almroth Wright, K.B.E. C.B. M.D. F.R.S.
The Rt. Hon. Lord Justice Younger, P.C. G.B.E.

Visitors.

James Henly Batty.
William A. Bone, D.Sc. F.R.S.
Alfred Carpmael, B.A.
Ernest Clarke, M.D. F.R.C.S.
Sigismund Goetze, M.R.I.
George H. Griffin, M.R.I.
John R. Leeson, J.P. M.D. C.M.
Sir Oliver Lodge, LL.D. D.Sc. F.R.S.
Percy A. Molteno, LL.M.
Sir Malcolm Morris, K.C.V.O. F.R.C.S.
Richard Pearce, F.G.S.
Hugh Munro Ross, B.A.
Sidney Skinner, M.A.
Thomas H. Sowerby, B.A.
William A. Tait, M.Inst.C.E. F.R.S.E.

WEEKLY EVENING MEETING.

Friday, May 5, 1922.

SIR JAMES REID, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Manager and Vice-President, in the Chair.

MICHAEL GRABHAM, M.D. F.R.C.P.

Biological Studies in Madeira.

THOUGH a mere speck on a map of ordinary size, the group of mountain tops comprising the archipelago of Madeira really occupies a considerable oceanic area; for the two main islands are quite twenty-five miles apart, and the various rocks in the district are separated by many miles of latitude and longitude.

Each island and each considerable rock has been almost certainly a distinct focus of volcanic ejection arising individually from the abysmal oceanic floor; each has its own loneliness and structure, and all the evidence is adverse to the theory that there was once another Atlantic continent of which these deeply-weathered and highly-sculptured hills and rifts survive from early Tertiary times. The suddenness with which each rocky shore passes beyond the hundred-fathom line into the profound Atlantic depth below is conclusive as to the distinct origin of each portion of the group.

The biological conditions, moreover, distinctly confirm the geological evidence in this respect; for taking the Testacea alone, of which the Madeira archipelago possesses quite 170 distinct examples, each island, each rock has its own peculiar examples; and of the total number of these pulmoniferous species, six or seven only are common to all the islands of the group. They have lived in segregation since the various and varying agencies of transport first brought them hither over the sea, and you may find them recent and with their cognate sub-fossilized ancestry now as when in past ages they were first and separately deposited on the profoundly separated land surfaces of the district. You might lower the Atlantic level a hundred fathoms without merging the component rocks of the archipelago in a common connection, and the evidence of the pulmoniferous gasteropods is at once copious and conclusive as to their separate volcanic origin.

The conditions around us here, in the British islands, are entirely different; for though the Scilly islands are as far from Cornwall as the northern Madeiran island Porto Santo from Madeira, the conchologist obtains no distinct species or even marked races in these

shells which are common to the islands and the mainland, for there has been here free access in past times.

We depend likewise on the evidence of shells and corals for our knowledge of the period during which the islands of the Madeira province were built up, deposits of these, once at the sea level and now high above the ocean, showing an Upper Miocene association. It is probable, however, that these mountain tops lifted their heads above the water in ages long anterior to the raising of the shelly beds from the seashore to their present altitudes of about 1,500 feet, and that the immense piling up of volcanic *ejecta* to form these islands 6,000 feet high in countless reiteration of eruption and lengthy periods of repose, began at least long before these fossils were living creatures on the Miocene shore.

The time taken in moulding the Madeiras to their present form we can only guess at.

The agencies of transport and distribution we know; the sea currents are the same: the same winds prevail: and birds, both migrants and stragglers, all potential carriers, still come and go—though it may be hard to imagine that these stupendous gorges have been carved out of the volcanic material by the busy little stream below leaping from rock to rock, or that the plants which clothe their mountain sides and represent in their variety almost all the known natural orders; that the Testacea already spoken of should have been established in 170 well-defined species; that nearly 700 species of the Coleoptera have been described, and that the presence of all these has been due to fitful and accidental influence, working sparingly now as formerly to bring so many forms of life to a group of completely isolated rocks in mid-ocean.

The forces of disintegration and erosion are equally difficult to fathom. In these regions there are no frosts or temperature excesses to disturb the placid flow of time or accelerate change, and thus after sixty years of familiar acquaintance with Madeira, it would be hard for me to point to a single rock or ravine as having appreciably lessened or deepened, though the rain of every winter carries away thousands of tons of material to the ocean and substantially wears away the land.

The archipelago came to us 500 years ago, so to speak, ready made, with every feature of its structure already well worn and with every species both of flora and fauna already specialized and established; and though the historical period of observation has been too short to show any definite variations in either indigenous or naturalized species, changes from ancestral forms are clearly indicated in the surviving fossils and semi-fossilized Testacea of the rocks.

The discovery of the Madeiras was due to the accident of a crazy vessel of Prince Henry the Navigator drifting from its course to West Africa during a severe storm in the year 1419. This Henry is the famous Portuguese prince to whom so much of the early

A Typical Water-Worn Gorge.

Residence of Christopher Columbus.

maritime enterprise of the Portuguese race is due, and to whose initiative in the very dawn of navigation the world-wide penetration of these people testifies. The Navigator's mother was an Englishwoman we may proudly say. But the discovery of the archipelago of Madeira was a stepping-stone to greater things, for here it was that Christopher Columbus, studying the charts of Peristrello, his father-in-law, a pioneer in Portuguese adventure, and an early Governor of Porto Santo, observed the flowing sea-currents and the evidence they brought of land and life beyond the Western horizon.

In reviewing the means by which the Madeiras were colonized by their rich and comprehensive flora and Testaceæ, we may at once omit the participation of any human agency in this work. The islands were untouched by man when first revealed to the Portuguese, even if we accept the legendary story of an English couple being cast ashore here a hundred years before the sailors of Prince Henry took possession, and it is extremely unlikely that the profound solitude and isolation had ever been violated by human feet.

It is, moreover, evident that the characteristic features in the local flora had developed long before the active volcanic forces of construction had subsided into the long period of slumber we are now experiencing: and in this connection I have examined with some care a fossil leaf bed on the north side of Madeira proper, whose preservation has been probably due to the subsequent protection of volcanic *ejecta*, forming in numerous strata a mound 120 feet high, the whole mass being capped with a vast mass of trachyte, which originally was in places quite 50 feet thick. The leaf bed, which was buried beneath all this mass in varying strata, contains well-preserved leaves of the local giant-leaved *Rubus*, the seed-cases of *Clethra arborea*, and other evidence of the stabilized evolution of the native flora in the early history of these rocks. One incident alone—viz. the complete disintegration and disappearance of the thick trachyte deposit which is now traceable only by markings on the adjacent columnar basalt—will fairly illustrate the time requirement for any important geological change.

You will notice by this specimen that the trachyte is a substantial stone of enduring quality, and in Funchal you may see stairways, columns, doorways and other lapidary work showing no material wear or disintegration after a century or two of exposure to the same conditions of erosion under which the stone cap of the Porto da Cruz leaf bed has wasted and is gone. Hence, taking the trachyte as indicative of the ebb of time, we need set no niggardly limit to the ages required for the establishment of the specialized forms of life presented to us in this district.

But if we know something of the ages which have been concerned in colonizing these remote rocks, we are none the less astonished at the completeness with which plants in their natural orders are represented in Madeira, and at the vast number of new and absolutely

peculiar species which have arisen in the islands from introduced primeval forms.

I can only in one brief discourse touch upon a few examples in the flora which are typical and representative, but unless these examples come from ancestors now long extinct I can suggest no satisfactory cause of their specific evolution other than environment, though their complete segregation would tend to preserve incipient features of variation and protect them from reversion due to crossing back into original stocks.

A period of 500 years is almost negligible in any geological time estimate; you have only to look along the Downs at Eastbourne to realize the stability of existing conditions, for we have here clear historical evidence that the surface of the chalk, with its thin film of covering soil, has undergone no tangible change since Cæsar fixed his camp there 2000 years ago, and we may reasonably assume both that the same conditions existed for many thousands of years previously, and that they may endure into an equally prolonged futurity. Let us reflect that since that mouldering Roman camp was peopled, the whole history of our race and civilization has been enacted, and that our literature, our traditions, our knowledge of things celestial and terrestrial and our faculties of appreciation have developed, and probably have culminated, during this minute portion of a transient episode of the earth's surface history. My desire in this geological digression is to convey to your minds an idea of the conditions, if time is a condition, involved in the evolution and stabilizing of the specific living forms which characterize the district I am representing.

It is less bewildering to believe that these special forms of life were brought to us in pots from the Garden of Eden than to trace their true descent in the dim and distant past from ancestral forms which no longer survive. The Testaceæ you can compare with ancient types in fossil beds, but the flora has no such satisfactory appeal.

I ask you to look upon these beautiful specimens of *Oreodaphne*, *Persea* and *Clethra* wood, whose stately trees still adorn our mountain side, and which originally gave the island the name of Madeira, in reference to the superb timber *materia* with which it was clothed. Total destruction of the surviving forests has been checked during the last 150 years by the introduction of the common *Pinaster*, which grows quickly on waste mountain ground and supplies fuel and wood for many purposes; and the scanty presence of conifers in the native flora has been recently substantially supplemented by the introduction of *Pinus Insignis*, *Cupressus Macrocarpa*, and many others which forty years ago I began to plant and whose importance is now fully realized.

A second *Persea—gratissima*—which you will know as the Alligator pear-tree, is now everywhere cultivated, and I have tried to increase its range of growth by grafting it upon the wilder *Persea*

A Littoral Echium.

Indica of the Madeira mountains, whose seeds I have also sent for the same purpose to the warmer regions of the United States of America.

The Coniferæ were represented originally only by two junipers and a *Taxus*: and the flora generally, in conformity with that of other oceanic centres, gives us numerous examples of orders with a single genus and of genera with a single species.

Let us for a moment glance at the striking Echiums of Madeira, and try to imagine their association with the pretty little Viper bugloss of our British meadows at this season, from which, or some ancient ancestor, the Madeira fruticose examples, *E. Candicans* and *E. Fastuosum*, have developed to adorn our cliffs and precipices in their striking beauty of vivid blue.

Side by side with one of these is fast becoming naturalized the stately unbranching *E. Simplex* of Tenerife, its straight stem, 8 to 11 feet high, crowded with small white flowers, and the two species remain, year after year, distinct and unblended.

Now, the common honey bee of Madeira, identical with the black bee of the British Islands, had become so destructive a pest in the vineyards seventy years ago that the authorities intervened and banished every hive away to the mountains, where, feeding on vast tracts of *Vaccinium*, and ceasing their depredations among the grapes, they would follow their lawful calling of fertilizing and honey-gathering. Then, in view of the importance of the bee in fertilizing, hearing that the Carniolan bee would not attack fruit, I started a heavy swarm among the vines of my Quinta. The flowering echiums were at once literally beset with the new bee, which seemed to prefer their nectar to any other food which our gardens provide in abundance. I watched anxiously for the issue of this invasion, and found eventually that the new bee had effected a remarkable hybridation of the Tenerife species with pollen from *E. Fastuosum*, and that the resulting hybrid, still mainly *Simplex*, had acquired feebly the branching habit of *E. Fastuosum*, a tinted flower, and in a languid manner the perennial vitality of the Madeira species. *E. Simplex* dies down after flowering at the conclusion of its biennial existence; but the hybrids survived long after flowering, and presented us with the most remarkable sight of their helicoid flower cymes—potentially indefinite—and normally 2 or 3 inches in extreme length, unfolding and unfolding, until, trailing on the ground, and still attached to their lingering parent by a thread-like stalk 7 or 8 feet long, ever with an opening flower, death happened at the root and closed the whole episode. The hybrids gave no fertile seeds, but for several generations *E. Fastuosum* showed traces of contamination with *E. Simplex*.

In general botany we recognize twenty or more species of *Echium*, but there is not one of these to which you can point as a probable ancestor of the fruticose forms which characterize the plant in the

Atlantic islands, neither in Madeira is there the shadow of any form intermediate with the specific kinds beyond this district: but the two indigenous species have long ago acquired a stability which shows no sign of variation or tendency to cross with newcomers.

The Carniolan bees, moreover, failed in the promise of their youth: and beholding the daughters of the land, that they were fair, took them wives of all they chose, and were speedily led astray into the evil ways of the local bee, to the detriment of my vineyard; and the last state of this man was worse than the first.

I published a detailed account of the *Echium* hybrid at the time when Miss North brought her charming drawings to Kew.

In a country where everything will grow there is a great temptation to introduce new plants, and many a flower from this country has proved a pernicious pest in Madeira. This is specially the case with species of *Oxalis Eupatorium* and *Senecio*, and many more, and we are now witnessing the ineradicable spread of the *Freezia Refracta*, which settles in every crevice and enchants us with its delicate fragrance.

Among the glories of the native flora is the *Ranunculus grandifolius*, a giant buttercup, with its intensely yellow flower, quite two inches in diameter, and with its huge characteristic, dark green enamelled leaf, growing also with masses of *Geranium Anemonifolium* and spikes of the yellow fox-glove (*Isoplexis*) of this region, all absolutely specific and unknown beyond the islands.

But all this is mere digression and beyond the scope of my discourse, for it is more profitable to speak in these days of colonial expansion of Madeira as a focus of dissemination in the spread of plants of economic value.

I ask you to consider the biology of the small *Sechium* gourd, which, coming from Brazil originally, has now been established in Madeira for a hundred years. The gourd is one-seeded, and is planted entire in the surface-ground, where, enlarging after the germination of the seed into an ever-expanding subaerial rhizome, the flesh surrounding the seed survives and becomes a part of the permanent plant growth. The plant is perennial in habit, throwing up annually, with little or no attention, countless stems which cover and closely mat a trellis work of three hundred square yards or more, whence hang in profusion the fruits of this valuable esculent. I know of examples which, planted forty years ago, are still in full vigour, yielding abundantly this favourite and agreeable vegetable. The *Sechium* is of priceless value as a food, and moreover, owing to the richness of its component salts, is pre-eminent in its power of assimilating fatty matter. But besides all this, the plant yields among its roots a substantial supply of tubers, which are hardly inferior to the sweet potato in importance, and the flowers possess an enchanting peach-like fragrance, which adds to the charm of its cultivation.

I have sent the *Sechium* far and wide, and I have the satisfaction of knowing that it is now well established as a potent contribution to the general well-being of many of our colonies.

During the war, when these Southern seas were infested with German submarine U-boats, which, without discrimination, wantonly destroyed everything, and completely cut off our food supplies, we had to learn the value of our local resources, and to economize them, and it was then that I fully realized the potency of the *Sechium* substance in promoting the assimilation of fats, whether by saponification or emulsion ; and I saw on several occasions the sullen apathy of incipient starvation awaken into animation under this effective influence.

A meal lacking in fat is deficient in staying power and output of energy ; and in times of scarcity it is of supreme importance to make the most of what is available. During the digging of the Panama Canal the Italian labourers had to be coerced into consuming a full fat ration in order that their daily work should equal the output of their well-fed Canadian associates.

It has also been my privilege to scatter the seeds of the small cherry tomato, an agreeable vegetable and a potent anti-scorbutic, over many an arid and ill-supplied district ; and I have lately learnt with pleasure of its secure establishment on those desolate remote rocks of the microscopic archipelago of the Salvages which, 150 miles to the south, have a Madeiran ownership. This plant, with its almost leafless, straggling stem, will mat whole valleys with its brilliant produce. The presence, in the Salvage islets, of the *Monizia Edulis*, the carrot-fern of Madeira, and unknown elsewhere, as well as a species of the *Apterous Deucalion*, otherwise only known on the Madeira rocks, side by side with the Canarian *Astadamya*—a specific samphire—seems to establish a certain correlation or agreement between the botanic and entomological distribution of species in these two island groups.

Some of our very interesting importations are sterile either under cultivation or in the absence of effective fertilizing agency. Before the year 1870 the garden terraces of Funchal possessed five or six striking examples of the bignoniaceous tree, *Jacaranda mimosifolia*. These, though abundantly pollenated by bees and humble-bees, were entirely sterile until a newcomer of the same species began to flower and yield its fertile pollen. The prevailing sterility then vanished, all the old trees bore abundant seed-pods, and there is now no garden or public walk which is not adorned copiously by the flowering blue tree which, with the yellow *Grevillea* and vivid *Erythrinas*, forms so startling an illustration of the acquired flora of Madeira.

No variety of fruiting banana has ever seeded in Madeira, though the bees freely visit their apparently perfect flowers, and pass, pollen-laden, from plant to plant. The small shrubby *Solanum* from Guatemala, fancifully named the melon pear, has likewise lost all fertility, and can be propagated only by cuttings. Here, too, is the

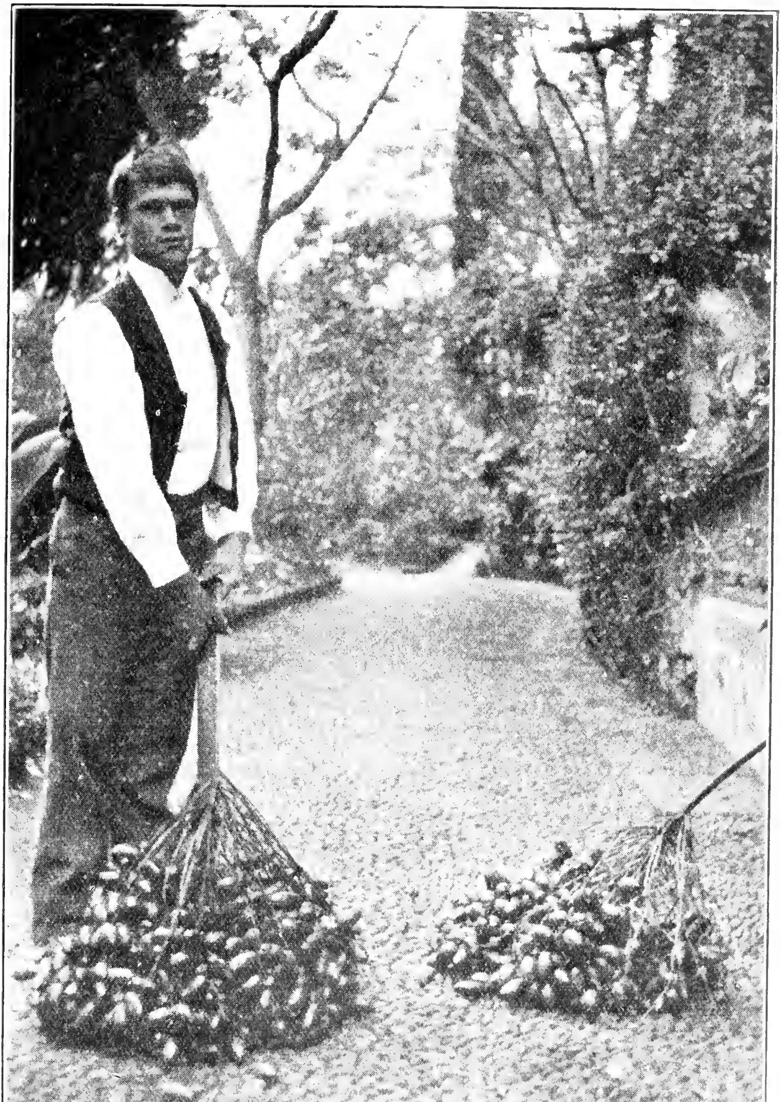
smaller *Strelitza*, waiting for a moth competent to open these folding doors and carry the concealed pollen to the inviting stigmas.

On the other hand, we are constantly plotting to curb the fertility of some of our desirable fruits. This is especially the case with the custard apple, which we select and graft from trees which bear few-seeded fruits, and I learn, with much satisfaction, that a new variety of Loquat tree, bearing a one-seeded fruit, has been successfully introduced into Madeira, and is now coming into bearing. The Loquat tree, the Japanese *Eriobotria*, grows in Madeira in weedy profusion, rendering the whole country fragrant with its flower perfume in November, and yielding masses of its attractive fruit from the beginning of March until almost the end of May. The British market should be flooded with Loquats in this fruit-vacant season, as also with the tasty broad-bean of Madeira, which travels to this country in perfection and can be supplied in profusion.

But the oceanic climate of the Madeira district, where so much of the temperate and tropical flora is blended, has been equally congenial to the numerous pests in plants and animal life which from time to time have been unwarily imported. Some of these, *Oidium* and *Peronospera*, we have learnt to cope with, and they are no longer formidable: likewise, too, with the *Phylloxera Vastatrix*, which in its apterous underground stages entirely destroyed the Madeira vineyards before its life-history was known in the late seventies of the last century, and has now become, as I predicted, far less active and almost negligible in its depredations; and thus the present supply of Madeira wine is once more adequate and even superabundant. I have reason to think, too, that some credit for restraining the virulence of the *Phylloxera* is due to the Argentine ant, whose presence has certainly coincided with the waning activity of that *Aphis*.

The Argentine ant arrived in Madeira about thirty years ago, and had become well-established before its presence was suspected, owing, I think, to confusion with the smaller *Œcophera pusilla*, then universally present and since completely ousted by the *Iridomyrmex humilis* of the Argentine.

In America, too, this *Iridomyrmex humilis* attained the very first rank among injurious pests before it was identified and its life-history was known, but it was very soon carefully studied, and nine years ago the United States Department of Agriculture issued a complete account of its life-history, its spread, and misdeeds, and also suggesting the means for arresting its progress. If it is true that the British islands are already invaded or threatened by this pest—and the ant is quite capable of withstanding the excesses of our climate—our own Ministry of Agriculture might most usefully reprint and circulate the American booklet, supplementing it with whatever has been written in England, together with my own paper on the subject read before the British Association two years ago.]



Stoneless Date Fruit.

In Madeira we have established a *modus vivendi* with the ant: we protect our food, our stores, our bees, and we spray our fruit trees, finding that the ant has no further interest in them when the *Aphis* and *Coccus*, which cater for it, are destroyed, and we have the satisfaction of seeing our citrus fruits returning and our coffee plants reviving. A very weak solution of corrosive sublimate is an effective restraint, and a narrow circle of potassium cyanide in powder at the foot of an infested tree will kill every ant coming and going in a short time. After treatment with the cyanide I found that 47,000 ants had been engaged in draining a single lemon tree of its vitality, and moreover that the destruction of the pest on this scale made no difference in the numbers which, in forty-eight hours, arrived to replace their defunct brethren in their destructive work on the lemon tree in unaffected activity. I could forgive much if the presence of the Argentine ant in Madeira had made any difference in the prevalence of *Pulex irritans*, or the common house fly, but in the larval state these creatures seem unattractive to the ant and have not abated, though in the adult state they are greedily pursued. I can conceive no foundation for a recent *Daily Mail* assertion that the birds of Madeira have been destroyed by the Argentine ant, for our gardens abound with every species, as in time past, and are melodious with the full-throated song of the wild canary, the melody of the blackbird, the robin and blackcap, as before the advent of the *Iridomyrex* interloper.

Far the most formidable of the few enemies of the ant in Madeira is the *Pholcus phalangioides*, a sluggish being with a cylindrical body and enormously long legs, well known in this country. This spider spreads an untidy, loosely-stretched snare in every corner, and is never without an ant in its mouth—the vast heap of dry-sucked skins beneath testifying to its restraining efficiency. Nevertheless there are signs that the first wild activity of the ant is now abating and that the pace of its increase is slackening.

The queens of this species are winged. The mating more often takes place in the formicary than in a nuptial flight, and after a very brief honeymoon, during which the female permanently discards her wings, she issues forth with the workers to found a new colony wherever the conditions of food and shelter invite, and, discarding the cares of motherhood, leaves the rearing of her prolific progeny to the zealous care of her neuter associates. In overcoming obstacles in search of food, the ant shows an intelligence and ingenuity which are truly surprising. Hundreds will voluntarily drown and create a bridge for the main body of assailants if they set their minds on attacking a colony of bees isolated by protecting water. So likewise, if they hear the buzz of a fly which has been caught on a sticky fly-paper, they will patiently bridge the sticky surface with grit or anything else they can carry until they arrive on a narrow pathway to seize and dismember the unfortunate captive. Occasionally you

may see a queen being groomed and tended by the workers, but for the most part she is her own lady's maid in her simple toilet and permanent widowhood.

But my discourse will degenerate into a lecture if I pursue this subject further.

I now invite you to accompany me down to the profound dark depths where the mountains we have been considering have their sturdy roots, and I will try to give you just information enough of the natural history of these southern seas to enable you to realize the character of the ocean floor itself, and of the creatures which live and thrive in those obscure cold regions under the extreme pressure of their environment.

But before we descend it is my duty, standing here—the duty of everyone engaged in oceanic research—to urge you to support us in our claim for further organized exploration of the ocean bed and its biology, even if alone in consideration of unexplored food resources and dormant supplies. Oceanic exploration belongs to us as a part of our Imperial expansion and responsibility, and you cannot study those forty-seven huge volumes which embody imperfectly the results of the "Challenger" expedition of forty years ago without the conviction of the need of further work. A second "Challenger" is long overdue, and the forms of many unknown living beings, their mode of life and their relations to other organisms, living and extinct, together with the phenomena of their geographical distribution and economic value, are waiting to be brought to light; and many a fishery, with its untold wealth and reserve of food, has yet to be discovered and applied to the needs of constant human expansion. Our lavish national expenditure should surely extend to the tiny cost of ministering to this aspect of the public welfare. The single-minded, unselfish efforts of scientific workers are really a true socialism, in which there is no more thought of personal advantage or enrichment now than influenced him whose widely-revered memory is still focused in this building—namely, Michael Faraday. But we have no organization to urge our claims with due weight, neither can we strike or otherwise coerce the Government. I will only add now that the discoveries of the "Challenger" expedition fully repaid the Government of the day for the cost of its equipment and maintenance.

I now ask you to consider this somewhat ungainly-shaped creature before me, which I have brought from the deep sub-tropical sea. The fish, *Polyprion Cernier*, is now very properly regarded as a typical example of the Serranidæ, and is essentially sub-tropical, though occasionally having a larger range. In the Madeira waters it is a common fish, and its interesting life-history has prompted me to bring it before you as a fair representative of our deep sea inhabitants. The fish, rare so far north, has been taken in the mouth of the British Channel, and is known as the wreck fish, from its

habit of accompanying floating logs of timber with their attached barnacles and other food, and it was long erroneously ascribed to the Stone Basses of Jamaica by a Cornish ichthyologist. The Cornish people, you know, are learned in pilchards, but the true identification of the monster before you was eventually established by Mr. Yarrell. It is singular, too, that so remarkable a fish, in size, form and food quality, now known to be quite common in the Mediterranean Sea, should altogether have escaped the notice of the early Greek and Roman naturalists, and indeed still more strange that there is no mention of it in the writings of Rondelet, Salviani, or Linnæus; but we have to remember that both in its habits and general aspects it might easily be confounded with *Labrax Lupus* and other near relations.

The sherny in Madeira is captured by the hook, and though small shoals of fish weighing from 5 to 20 lbs. are freely taken near the surface, or 20 fathoms beneath, the proper habitat of the full-sized creature, weighing up to 100 lbs., or even much more, is well away in the open sea, at the enormous depth of 2000 to 3000 feet. The fishermen weight the end of their line with a stone, and bait the last 50 feet at intervals of about 18 inches with mackerel flesh attached to strong hooks. Brought up suddenly from so great a depth, the compressed air within the fish expands and so distends the fish on the removal of the vast pressure below that it rises to the surface not indeed dead, but wholly powerless in a sort of cataleptic spasm, with its stomach or some large fold of it inverted and protruding from its mouth like a huge bladder, and with its eyes forced in front of their sockets. In the last 100 feet or so of the ascent, the captured fish rises faster than the attached line can be drawn in, shooting quite out of the water at its first emergence like a cork or a bladder, from the lightness caused by its vast distention. The sherny is spawned in the warm surface water of the open sea, and lives its early life in the sunlit depths, but I can give you no clue as to the conditions which prompt the fish eventually to descend into the dark, cold and abysmal depths whence the larger examples are only to be met with; neither have I so far succeeded in obtaining any distinct idea of the age of the sherny in its varying size by scale growth or markings. The deep sea examples arrive at the surface fat, plump and well fed, and thus abundantly confirm our experience that the sea is not azoic at any depth.

In general character the sherny is an ungainly fish, with a capacious gape, an enormous head, and eyes surprisingly small and dull when compared with the highly-coloured brassy lustre of the huge eye of some of the *Berycidæ*, which also live in deep water. Nevertheless the dull covering of the sherny is shared also by the *Prometheus Atlanticus* and *Aplurus*, which live in the same association in those stupendous depths, and contrast surprisingly with the brilliant hues—silver, scarlet, rose and purple—of the *Scorpena Lampris* and

Sebastes, which live in equally deep water, though seldom in close association with their leaden-coloured brethren. No one, however, who has not assisted at the capture of these abysmal inhabitants can have any true idea of the gorgeous tints of the dullest of these creatures on first emerging from the sea.

The sherny has a very large bladder firmly attached to the spine, and a few words on the nature and use of the air-bladder generally will not be out of place here. The function of this organ is as yet by no means clearly made out. You will read of them as swim-bladders, especially as relating to the carp and other fishes frequenting superficial waters, which are believed to regulate their submersibility by a voluntary act of filling, compressing or emptying the bladder of its contained gas. But the physiology of the organ is by no means so simple as this. It varies much in size. Here it is large, but in the *Aplurus*, or oil fish, which lives in company with it and feeds on the ocean floor, it is completely absent. The air-vessel has, indeed, in many cases a pervious duct opening into the intestinal canal, but the duct is often absent altogether or obliterated into a solid cord in the full-grown fish, leaving the bladder a closed sack. The bladder-walls truly contain muscular tissue adequate to the compression or expulsion of the gas if there is a hollow duct and exit, but as the vessel can only be again distended and filled by the gradual secretion of gas from the blood, we may fairly assume that the voluntary regulation of the specific gravity of the fish is, to say the most, a subordinate portion of the bladder functions. On the other hand, the extremely vascular development of the lining membrane into a lung-like anatomy points clearly to pulmonary functions, and it is quite likely that an interchange of gases may take place between the two kinds of blood traversing the retia mirabilia in many fishes. Moreover, and thirdly, in regard to the physiological functions of the air-bladder, the position of the viscus, its firm attachment to the spinal bones, and its prolongation upwards to connect in some examples through a chain of ossicles with the organ of hearing, seem a sure indication of the use of the organ as a resonator in the conveyance of sonorous vibrations to the auditory focus. The gaseous contents of an air-bladder vary considerably, consisting mainly of nitrogen in many instances. But after examining the bladder of eighteen large examples, presumably from very deep water, during the last few weeks, I am unable to confirm the current idea that the viscus is charged with oxygen in the abysmal depths ; nor is it probable that a fish could afford to secrete or excrete a gas so vital to its welfare.

Speaking generally, the consumption of oxygen by fishes is comparatively small, especially in regard to tribes inhabiting stagnant waters—a man, for instance, absorbing many thousand times the amount required by a tench. But with the active *Scrombidæ* the requirement is of course greater, and the fish has often a higher temperature than the water in which it swims.

It is well known that the oceanic fishes of deep water have a low standard of respiration, a high degree of muscular irritability, and that they sustain life long after being taken out of the water. The sherny heart will go on beating for many hours after every other sign of life has ceased, and I have noticed the same remarkable survival of movement in the heart of the *Zygena Malleus*, the hammer-headed shark, after I had completely severed it from a recently captured monster.

The sherny which we have been considering with many associated species does not come to us in rare, solitary examples, but is present in perennial supply, and the impression left after contemplating our fish markets, with their abundance and variety, and noticing the patient, ingenious and primitive methods employed in capture, is that we have in these southern waters a wealth and reserve of food which, in the progress of investigation, can be made to minister to the needs of regions far beyond the requirements of its present application.

It would be both profitable and intensely interesting, by sinking basket traps, such as are in use in lesser depths, to the abysmal region, 3000 feet below the surface, to get an idea of the stationary conditions of life below, and thus intercept many inhabitants which would escape the dredge capture ; but as these traps should remain at the bottom for twelve hours there is difficulty in carrying out such observations, for one must personally investigate the take as it emerges at the surface.

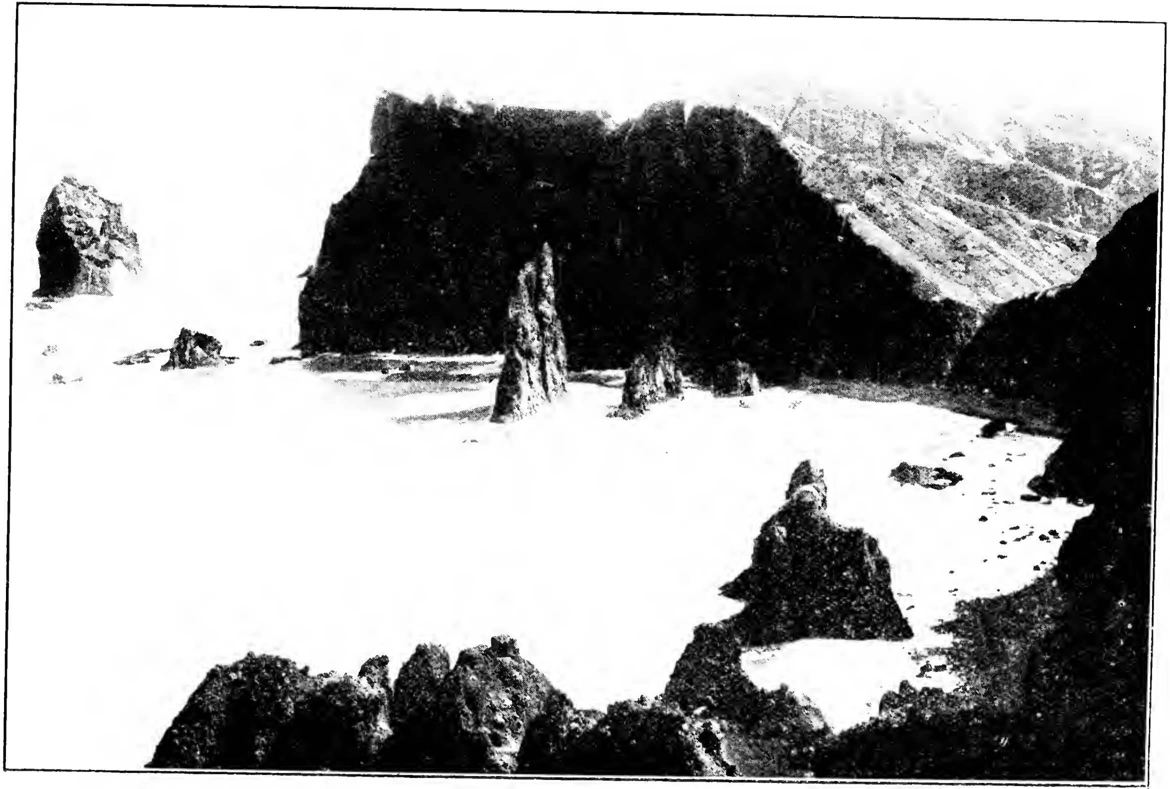
But the whole subject of these super-equatorial marine food resources abounds with problems both biological and economical.

The surface plankton and the living creatures in the adjacent layers of sunlit water have yet to be explored, if only as a condition precedent to the estimation of deep sea values, for we have in the abundance of the surface food a key to the welfare and maintenance of life in the depths. The *Globigerina* ooze on the surface below is ample evidence of the perpetual rain of food which must be ever descending from the surface. Much of such food is doubtless intercepted and consumed in its fall, but by far the greater part either reaches the bottom or is actually dissolved in sea-water as it falls, becoming thus a tangible source of deep sea sustenance. Nevertheless, the robust condition in which our deep sea fish arrive at the surface when captured, and the numbers in which we know them to abound, suggest that we are by no means cognisant of all the nutriment which is available in those dark abysses. I have recently been observing the stomach of the sherny in its various stages during the evisceration of the fish in the preparation for its sale in huge transversely cut steaks in the public market of Funchal. The smaller surface examples always show a fair quantity of food debris within them, but there is a marked difference in the stomach of the full-grown abysmal fishes. The stomachs of these creatures are almost

empty or nearly so, and the walls are tough and contracted, as though in the placid depths and conditions of less active life and movement the fish relied on the absorption of much nourishment from the substantial supply which may be held in solution.

The fish, *Aphanopus carbo*, before me, with its formidable dentition, is one of the Trichinuridæ, or as occasionally seen on our southern British coast, the Scabbard fish. The creature has a wide range, and is known as the Frost fish of New Zealand. In Madeira, though from its anatomy a true deep-sea fish, the *Lepidopus* is taken at all—except in the very deepest recesses—depths in surprising numbers, ranging freely among the inexhaustible invertebrate population of the intermediate waters, where at 1000 feet below the surface the Portuguese fishermen sink large basket traps, and bring up a perennial take of the *Pandalus* prawn I here show you, with an accompaniment of Amphimonidæ in vast numbers. The depth of 1000 feet is quite sufficient to produce the destructive expansion suffered by the fish of the deeper sea, for no single prawn or creature long survives its sudden transference to the surface. You will observe the enormous development of the sensitive prawn antennæ and their deep red protective colouring.

The condition of the ocean floor itself is not less mysterious than the life-history and food of its occupants, and from time to time vast commotions occur of which at the surface we have little evidence. One of these came to my mind in the following manner. I was engaged at the time in observing the strength and variations of those so-called earth currents which inductively traverse our deep sea cables and are never entirely absent. My hope was to establish a definite relation between these submarine indications and the earth tremors which are registered by seismological methods, and thus to render it possible not merely to record something which has already happened, but to predict a coming commotion by reference to augmented activity in the cable inductive movements. While these observations were in progress we were startled by an agitation which lasted with increasing intensity for four days and then suddenly ceased altogether. The cable placed at my disposal had broken, a great landslide had taken place on one of the Dezerta rocks eastward of Madeira, and we were bewildered to determine whether the landslide had broken the cable or an earthquake had been the cause of both. My fellow-worker agreed with me that our exaggerated earth-currents had been premonitory, and that the floor of the ocean in the path of the cable had been broken up volcanically, but it was only when the repairing cable ship arrived, and took soundings, that our deductions were verified. It was then found that many miles of the submarine cable had disappeared and were lost, and that the ocean floor had been so broken up into mounds of jagged rock that it was necessary to depart 15 miles away from the direct course before an undisturbed bed could be found for the reception of the new line. We should have



Rocks in Deep Sea Setting.

known nothing of all this but for the accident of the telegraph cable crossing the district of this submarine commotion. Knowing all this, I was intensely interested in hearing, two years ago, at the British Association, Professor Herdman's account of the destruction of a fishery in the sub-tropical waters of the United States. The narrative, full of interest, runs thus :—

The capture of a solitary fish, a new species of the Tile fishes named *Lopholatilus Chamæleonticeps*, led to the discovery by the local authorities of a well-stocked fishery which for two years substantially augmented the fish-market supplies of a long stretch of the American coast. Then, in Professor Herdman's words—*something happened*, and the sea surface for hundreds of square miles was strewn with dead Tile fish in amazing quantities. The authorities again sent to investigate the ground, but not a single fish was taken, and it was found moreover that the destruction had extended even to the invertebrate inhabitants of the same district. An attempt to account for the catastrophe by a suggestion of a sudden change of temperature due to a deflection of sea currents seemed to me unsatisfactory, and in the light of what happened at Madeira it seems more highly probable that certain fissures were volcanically opened, and that the water was poisoned by the issue of noxious vapours without any surface evidence of what happened but floating myriads of dead fish.

I can go no further in this branch of natural history, though I could fascinate you with a sketch of many anatomical details, and especially of heart structure and function.

You must go to Madeira for any full and comprehensive appreciation of sub-tropical ichthyology. You can take part, if you will, far away in the open sea, in the exciting capture of our huge, voracious tunny fish ; you can study inshore the shoals of grey mullet, shapely and silvery, in their search for fresh water ; you can see our full-tinted red mullet, brought out of shallow water in unbaited basket traps into which they have aimlessly wandered : you can see the deep blue water tinted with the reflected purple of inviting rock fish ; or you can study in our markets the daily display in vivid colouring of *Sebastes*, *Scorpoena*, *Beryx* and *Polymixia*, interspersed with many a turtle, octopus, and strange crustaceans in surprising confusion. And all this will be doubly interesting if it evokes in you a desire to study and increase knowledge ; for it is clearly our duty to continue the work of investigation and classification which hitherto has been mainly due to our own countrymen. And surely a sense of responsibility attaches to us when we are reputed by the races of the South to be the custodians and exponents of science in general. We are lucky, indeed, if we can justify one-tenth part of the knowledge attributed to us.

But apart from any such consideration there is a satisfaction and charm in the pursuit of any branch of natural science for its own sake, which will grow and expand with experience. But every man

should have his chance, and elementary education would ensure that the small percentage of the population with any special aptitude or desire to excel should not be lost for want of opportunity. No one can guess where exceptional endowment may be found, but whether in the cottage or palace, whether choked in luxury or starved in poverty, it is the duty and interest of the community to discover the potential genius and to place him where he can accomplish that for which he is fitted. In science a full output is expected; one may work overtime; and there are none of the suppressing restrictions of a false communistic socialism or cramping of individual effort. Pre-eminence is accepted and encouraged, for it is obvious that one star differs from another in glory. We can all contribute to progress, even if we differ in opportunity and mental equipment, and in all cases we very soon realize the breadth of the old saying: "Magna opera Domini; exquisita in omnes voluntates ejus."

[M. G.]

GENERAL MONTHLY MEETING,

Monday, May 8, 1922.

Sir James Reid, G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

John Philip Blessig,
William Dobinson Halliburton, M.D. LL.D. F.R.S.
E. H. Hankin, M.A. Sc.D.
Mrs. Percy McQuoid,
Samuel Thomas Nunn,
Mrs. Theodore Stephenson,

were elected Members.

The Chairman announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

John Mitchell Bruce, C.V.O. M.D. LL.D.
Sir Ernest Moon, K.C.B. K.C. LL.B.
The Hon. Sir Charles Parsons, K.C.B. J.P. LL.D. F.R.S.
Sir James Reid, Bart., G.C.V.O. K.C.B. M.D. LL.D.
Sir David Salomons, Bart., D.L. J.P.
Sir J. J. Thomson, O.M. LL.D. D.Sc. F.R.S.
The Right Hon. Lord Justice Younger, P.C. G.B.E.
Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S. (Treasurer)
Colonel E. H. Grove-Hills, C.M.G. D.Sc. F.R.S. (Secretary)

Sir Joseph John Thomson, O.M. LL.D. D.Sc. F.R.S., was re-elected Honorary Professor of Natural Philosophy.

Sir Ernest Rutherford, D.Sc. LL.D. F.R.S., was re-elected Professor of Natural Philosophy.

The Chairman announced the decease of Sir William Phipson Beale, Bart., on April 13, and of Sir Alfred Bray Kempe on April 21, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to place on record their sense of the great loss the Institution has sustained by the death of Sir William Phipson Beale, Bart., K.C. F.C.S. F.G.S., President of the Mineralogical Society, late Manager and Vice-President.

Sir William Phipson Beale studied Chemistry under Bunsen at Heidelberg, where he worked alongside of Roscoe, Mond and Messel, who became

chemists of distinction. In order to gain a thorough knowledge of Scientific Metallurgy he proceeded to Freiburg University, and later on to the French School of Chemistry at Paris. His studies in Applied Chemistry were afterwards diverted for the Bar, and he became a Bencher of Lincoln's Inn in 1867. He was a distinguished Counsel at the Parliamentary Bar, and later on became a Member of the House of Commons, where he represented Ayrshire for a period of twelve years. On retiring from professional practice he again took up his early scientific studies, and published an original work entitled "Introduction to Crystallography."

During his twenty years' Membership of the Royal Institution, both as Vice-President and Manager, his scientific and forensic capabilities were of great service in promoting the objects of the Institution.

The Managers desire to express on behalf of the Members their sincere sympathy with the family in their bereavement.

RESOLVED, That the Managers of the Royal Institution of Great Britain desire to place on record their sense of the great loss the Institution has sustained by the death of Sir Alfred Bray Kempe, M.A.(Cambridge), D.C.L.(Durham), F.R.S., Bencher of the Inner Temple, Chancellor of the Dioceses of London, Southwell, St. Albans, Peterborough, Chichester and Chelmsford, Secretary to the Royal Commission on Ecclesiastical Courts. Sir Alfred Kempe's scientific work was recognised by his election to the Royal Society in 1881. He became Treasurer in 1899, which office he held till 1919. The author of many Mathematical Memoirs, some of which have been recognised with much distinction.

Sir Alfred Kempe was a Member of the Royal Institution for fifty years, and always displayed a personal interest in the welfare of the Institution. As Vice-President and Manager he rendered great assistance by his forensic and legal knowledge in the settlement with Dr. Ludwig Mond of the Conveyance and Deed of Trust of the Davy Faraday Research Laboratory of the Royal Institution.

The Managers desire to express on behalf of the Members their sincere sympathy with Lady Kempe and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, May 19, 1922.

SIR J. J. THOMSON, O.M. M.A. LL.D. D.Sc. F.R.S.,
Honorary Professor of Natural Philosophy R.I.,
Vice-President, in the Chair.

SIR WILLIAM BRAGG, K.B.E. D.Sc. F.R.S. M.R.I.

The Structure of Organic Crystals.

IT may be said with truth that modern advances in physical science are due in the main to the acquisition of the power to handle the individual atom. Until the present time we have always attacked the problems of matter by examining the behaviour of atoms or molecules in groups. The new powers arise in two ways :—

In the first the individual atom is endowed with excessive speed and energy, and is able to make its individuality felt on this account. The α-particle of the radioactive radiations is a helium atom moving with a speed of the order of one-tenth of that of light. While in possession of the relatively tremendous energy which the speed implies it can, unaided, make a visible impression on a fluorescent screen. It can pass through thousands of other atoms without sensible deviation, and if occasionally it suffers violent deflection it has penetrated to the very core of the atom which has deflected it. Rutherford has shown us what important deductions can be drawn as to the construction of the atom by examining these rare and sharp deviations, and is going even further in examining the shattering effect which the deflecting atom may itself experience. So also, the electron endowed with sufficient speed can transverse matter and bring about ionisation and other effects of great interest, but if its velocity becomes less that one million metres per second this free existence disappears. It is attached to the first atom it meets.

The second method of attack upon the individual atom proceeds on very different lines. It is by way of the mutual action of X-rays and crystals. When we are examining things by eyesight we follow the influence of the objects that we look at upon the waves of light. If we wish to penetrate deeper into the minute, we take advantage of the optical effects of lenses and build microscopes : but, even then we cannot attack individual objects containing less than many thousands of individual atoms. A limit is set by the difficulty that light cannot show us the form of things which are much smaller than the wave length of the light itself. With the aid of the very short

waves known as X-rays we can make our way down to objects ten thousand times smaller, but by itself this extension of our powers would be inefficient, because the effect due to one atom or one unit of pattern would be inappreciable. Here lies the value of the crystal, which, being an aggregate of some small atomic pattern repeated again and again through space, shows up on a measurable scale the properties of the atoms in the single unit. By the combination of X-ray and crystal we can examine the very foundations of material construction. It is difficult to set a limit to what may be the consequences of the exercise of these powers since we can now examine all physical effects, so to speak, at their source, and must in the end be able to refer all the physical and chemical properties of materials to the properties of the individual atoms and their mutual forces. So far the new methods have scarcely begun to show their full strength. A few inorganic crystals have been examined with a view of discovering their structure, but the new field of research is barely entered. Inviting roads lie before us pointing in numerous directions.

Very little has yet been done in the way of applying the new methods to the structure of organic crystals, although no study could be more tempting. Their vast variety of form, the perfection of their structure, their importance, all urge us forward, and especially the fact that the whole progress of organic chemistry shows that the science depends upon laws of position with which the X-rays are especially qualified to deal. The difficulty at the outset lies in the complexity. In the naphthalene molecule there are eighteen atoms: in what way can we expect by means of X-rays to solve the intricate problem of their relative positions? Our first attempts to solve inorganic crystals depended for their success upon two facts:—

The first, the simplicity of the structures which were attacked.

The second, the guidance derived from the principles of crystallographic symmetry.

The determination of the structure of rocksalt opened a way to further determinations of such simple crystals as the diamond, zinc blende, fluorspar, and others. In all these the principles of symmetry supplemented the knowledge derived from the examination of the intensities of X-ray reflection by the various crystal planes. As the work has proceeded in the hands of observers in many countries, other principles have emerged or are emerging which render further and very valuable aid, so that problems appear to be coming within our grasp that not long ago seemed most difficult of solution.

Of these principles, one began to appear in consequence of the very earliest results. It was a very striking fact that in crystals of polar substances the molecule seemed to disappear: it was in fact dissociated, and the structure of the crystal depended upon the grouping of the positive ions round the negative and of the negative ions round the positive. In rocksalt each metal atom is surrounded by six atoms of chlorine and vice versa. If we accept this as an

indication of the general character of such structures, adding to it the condition that every atom is to be like every other atom of its own kind in respect to relative distances and orientations of all its neighbours, it becomes possible to foretell the probable form of structure, using the X-ray methods for subsequent verification. This method of proceeding may be very much easier than if it were taken in the reverse way. We might, for example, have gone far to foretell the structure of fluorspar. It is an ionic compound in which the calcium atoms are doubly charged and fluorines are singly charged. Each positive is to be surrounded, therefore, by twice as many neighbours as each negative by positive. The fluorspar structure in which the metal atoms are arranged at the corners and the face centres of the cube, while the fluorines lie at the centres of the eight small cubes into which the larger ones can be divided, is one of the very few regular ways in which this numerical relation of 2 to 1 can be carried out. So also in ice, the 2 to 1 arrangement is carried out in a second of these ways, the relative numbers being 4 to 2. It is the lightest and most open of the 2 to 1 structures, and is consistent with the low specific gravity of ice and with the possibility of compressing the substance into denser forms: at the same time it shows the six-pointed arrangement and the featheriness of the snow crystal.*

The earlier results at the same time showed that in the diamond we had a construction of very different properties and nature. Here the atoms are electrically neutral and are bound to one another, not by electrical attraction from centre to centre, but by a more intimate process which probably consists in some way of a sharing of structural electrons. The diamond is on this account the hardest of known substances.

These considerations amount to a recognition that the bonds between the atoms may be of very different characters, though it may be difficult to draw hard-and-fast lines between them. We can say that there is a very strong electron-sharing bond of which the diamond is typical, and that there are ionic bonds in polar compounds which in general are of a weaker character, as, for example, in rocksalt, though on the other hand they may be strong when, as in the ruby, the ionic charges are large.

Lastly, there is a third type, which is found in the organic crystal, where it would appear that the separate molecule can be distinguished. The atoms in each molecule are strongly tied together, but the forces that bind molecule to molecule may be described as residual. They would appear to be weak fields concentrated at definite points on the molecule, the positive and negative charges to which they are due lying within it.

The second principle, which emerged fairly early in the experiments, was described by my son in an address which he gave in this

* Proc. Phys. Soc., London, vol. xxxiv. pt. 3, p. 98.

Institution some time ago.* We may call it the principle of radii of combination. The distance between the centre of one atom and the centre of a neighbour in many cases can be measured with great accuracy; we can compare these distances when substitutions are made in isomorphous compounds. The replacement of fluorine by chlorine, chlorine by bromine, bromine by iodine in a series of salts produces changes in the distances which imply that the radius of any one of the atoms mentioned may be treated as a constant within the range of the substitution considered. The accuracy is amply sufficient to give useful assistance in crystal analysis. It would not be true, however, to say that each atom has an invariable radius, and indeed the original statement of the principle purposely refrained from going so far. It is not right to speak of *the radius* of an atom; it is better to speak of *a radius of combination.* We may take an illustration from the behaviour of arsenic, antimony, and bismuth. The crystals of these substances are trigonal in form,† plainly showing that the properties of each atom are not the same in all directions within the crystal; in fact, analysis shows that each atom is fastened to three on one side of it by much closer bonds than to three atoms on the other side. One bonding resembles more closely that of the diamond, the other that of a metal where free electrons keep the atoms together by electrostatic attraction. It may be said that the atom behaves as a metal on one side and a non-metal on the other. At any rate, there are two radii of combination varying with the nature of the bond. The metallic bond is the weak one, and the cleavage plane cuts only through such bonds. It seems very likely that in this way we can understand the formation of crystals of different type when these elements enter into their composition. For example, in the cubic form of senarmontite (Sb_2O_3) the atoms of antimony are completely separated; each touches six atoms of oxygen, while each oxygen touches four atoms of antimony. Antimony is here behaving as a metal only, so that we represent it in a model as a sphere, and the uniform spheres of antimony and of oxygen naturally build into a simple crystal. It is a cube in which the atoms of antimony occupy the corners and centres of the faces, while the six oxygen atoms lie at the centres of six of the eight small cubes into which the large one can be divided.

There is, however, an alternative form of Sb_2O_3 known as valentinite, which is ortho-rhombic. Analysis, so far as it has gone, though it is not yet complete, points emphatically to the conclusion that here atoms of antimony are pairing, the bonds between the members of a pair being of the stronger variety already referred to.

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* See Phil. Mag., Aug. 1920.

† James and Tunstall, Phil. Mag., Aug. 1920 and July 1921; Ogg, Phil. Mag., July 1921.

We now have an elementary body of a dumb-bell shape which, when forming part of the crystal structure, will naturally cause a deviation from a simple cube.

Again, there are principles which are barely established as yet, though it seems probable that they will be found of material assistance in analysis. The greater expansion of some crystals in certain directions than in others seems to depend upon the nature of the bonds. Bismuth expands more along the axis than across it, as we might expect from the fact that in the one expansion the weak bonds alone can be operative. In the same way diamond has an extremely small expansion co-efficient because all the bonds are of the strongest kind, but in graphite, on the other hand, the expansion along the axis may be described as enormous. Mr. Backhurst finds an increase in length of 3 per cent. for a rise of 900° C. At the

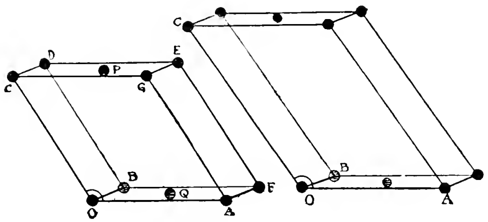

FIG. 1.—Unit cells of naphthalene and anthracene drawn to the same scale.

	OA = a.	OB = b.	OC = c.
Naphthalene	8·34	6·05	8·69
Anthracene	8·7	6·1	11·6

Naphthalene α = BOC = 90°, β = COA = 122° 49′, γ = AOB = 90°
Anthracene α = BOC = 90°, β = COA = 124° 24′, γ = AOB = 90°

same time, so far as can be inferred, the expansion across the axis is still quite small. In one case weak bonds only are concerned, in the other strong bonds of the same kind as in the diamond.

It is when all these considerations are taken into account that it seems possible to make an attempt upon the structure of the organic crystals. They are, of course, very complex; naphthalene contains ten atoms of carbon and eight atoms of hydrogen, and our ability to interpret X-ray evidence, that is to say, the relative intensities of reflection by the different planes in different orders, is not sufficiently advanced to place so many atoms in their proper position in the cell from this evidence alone. We can readily find the size of the unit cell, show that there are two molecules in it, and that the points, each of which represents a whole molecule, are to be placed as is shown in Fig. 1, but without some further help we can frame no hypothesis on which to proceed.

Suppose now that we compare the structures of diamond and graphite. As my son showed long ago, the structure of graphite must be derivable from that of the diamond by separating to nearly double their previous distance the sheets of atoms parallel to one of the cleavage planes of the latter crystal. The question has been very carefully considered more recently by Hull in America and by Debye and Scherrer on the Continent in the hope of finding more exactly the details of the movement; they do not quite agree. Fig. 2 represents the change as described by Hull. The bonds between the atoms in each sheet are unaffected apparently, but those between sheet and sheet are replaced by something much weaker. The diamond is typical of hardness: the graphite is used as a lubricant. If the hexagonal rings of which the sheets are formed have survived this violent change, why not suppose that they may survive the further change when the sheets break up into ring structures?

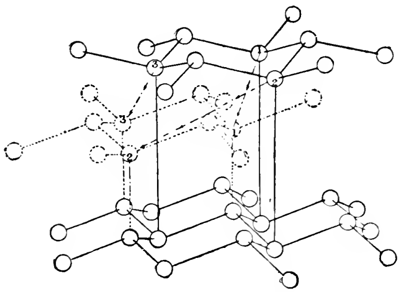

FIG. 2.—The fine lines of the diagram show the structure of graphite. By moving the top layer to the position shown by the broken lines the diamond structure is obtained.

In other words, suppose that the benzene ring is really a fact, not merely a diagram; the distance between atom and atom in the ring is 1·54 Å.U., as in the diamond, and perhaps we may add that the atoms are not all in one plane, but are arranged as may be seen in Fig. 3. We then proceed to test this hypothesis by finding whether we can fit together molecules of the assumed size and shape into the cells which hold them. From X-ray studies we know the exact form and dimensions of the cells, and can learn also much concerning the relative distributions of the molecules within them. It appears at once that in the few simple cases which have been examined an excellent fit is possible, and, more than that, we find encouraging signs that the structural idea has been chosen rightly. For instance, the comparison of the cells of naphthalene and anthracene, one a two-ring, the other a three-ring combination, shows that two of the axes of the cell remain constant, while the third has grown by an amount

which is nearly the width of the benzene ring. From these and various other indications we build a structure such as is represented in Figs. 3 and 4. It would seem that the molecules are linked

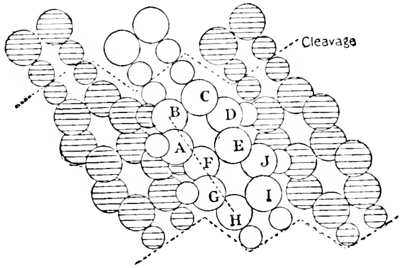

Fig. 3.—Showing mutual relations of three naphthalene molecules and parts of others.

The unshaded circles between the two cleavage planes represent a molecule as at Q (Fig. 1). The shaded represent molecules B and F in the same figure. The small circles represent hydrogen atoms, but their size is uncertain.

Diameter of carbon atom = 1·50. BH = 4·92. Projection of AD on the plane of the diagram = 2·50. Benzene ring consists of atoms A–F only.

together side by side more strongly than from end to end, and that is why these and similar crystals cleave across the end or β position.

If we examine α-naphthol in which hydrogen at the side of the naphthalene molecule has been replaced by an OH group, we find

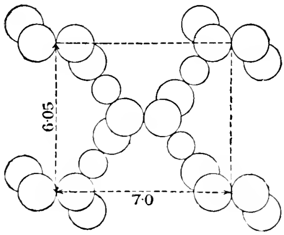

Fig. 4.—Section of naphthalene cell perpendicular to the axis of c, showing α-hydrogens connecting the molecules side to side.

that the standard cell contains four molecules, which is what we should expect, for each of the four α positions must be represented. When the OH group is taken from the side and put at the end, we find that the cell has shrunk sideways and grown lengthways by the amount we should expect to result from the addition of an oxygen

atom. When as in acenaphthene a complex group of atoms is attached to one side of the molecule, and the crystal to our surprise becomes more regular than before, right-angled instead of oblique, we find an explanation in the fact that there are now four molecules within the cell instead of two, and that by sloping in pairs in opposite ways they increase the symmetry of the crystal.

These examples may serve to show how an attempt may be made to arrive at a knowledge of the structure of these organic compounds with, I think, some success. It seems justifiable to see in the rigid and queerly shaped molecule attaching itself at definite points, and with great precision of orientation to neighbouring molecules, a cause of the immense multiplicity and, at the same time, the accurate form of organic crystals, and indeed to find here the foundations of organic chemistry.

[W. B.]

WEEKLY EVENING MEETING,

Friday, May 26, 1922.

Sir David Salomons, Bart., D.L. J.P. F.R.A.S.,
Vice-President, in the Chair.

W. E. Dalby, M.A. B.Sc. F.R.S. M.Inst.C.E. M.I.Mech.E.

The Internal Combustion Engine: Its Influence and Its Problems.

THE INFLUENCE OF THE INTERNAL COMBUSTION ENGINE.

To engineers the terms horse-power and horse-power hour have strictly technical meanings. They can be illustrated by comparing the weight and efficiency of an aircraft engine and a locomotive engine. An aircraft engine can be built with about $2\frac{1}{2}$ lbs. of metal per horse-power as against approximately 250 lbs. of metal per horse-power in a locomotive engine. An aircraft engine requires about $\frac{3}{4}$ lbs. of fuel oil per H.P. hour as against 3 lbs. of coal per H.P. hour used by the locomotive engine, in addition to which the locomotive engine must carry about 3 gallons of water per H.P. hour. All these, of course, are round figures. It is the extreme lightness of the petrol engine in relation to its power which has made it possible to develop aircraft.

An internal combustion engine of the Diesel type is built to use heavy oils, and it is the prime mover which led to the rapid development of the submarine during the war. Thus the internal combustion engine helped to sink our food ships, but at the same time helped to save us by driving the agricultural tractor. Few, perhaps, realise how serious was our position in 1917. Horses were required for the Army and were being taken from the farms; but the agricultural tractor replaced them at the plough and thus made it possible to maintain the necessary food supplies.

Probably the greatest effect of the internal combustion engine on our national life is its influence on road transport. Standing at Hyde Park Corner twenty years ago a motor car would have excited notice; standing there to-day a horse-drawn vehicle is rarely seen. The internal combustion engine is displacing the horse from the streets, and is even causing the railway companies grave concern. The chairman of one company stated at the last half-yearly meeting that the companies had lost 9 million tons of goods, and 6 million passengers to the motor lorry and the motor car. This is a remark-

able achievement for the small internal combustion engine fitted in these vehicles. During 1921 about 800,000 licences were issued to vehicles propelled by internal combustion engines, and the tax on them amounted to about 10 million pounds.

These brief considerations indicate how profound has been and is the influence of the internal combustion engine in shaping our destinies. It has conquered the air, and given us a prime mover useful in farming and in transport. It is influencing the policy of our railways, and will shortly so transform our outlook and our modes of life that men of to-day will appear to be separated from their boyhood not by a few decades but by a few centuries.

SOME PROBLEMS OF THE INTERNAL COMBUSTION ENGINE.

Considering combustion from the point of view of the Kinetic Theory of gases, but without attempting to explain the nature of the differential attraction between molecules, most of the energy developed in the cylinder of an internal combustion engine arises from the fact that oxygen combines with carbon and hydrogen to develop large quantities of heat. The function of the engine is to convert as much as possible of this heat into mechanical work.

It can be deduced by the laws of gases that the molecules at 22° C. and atmospheric pressure require 729 times the volume they occupied as a liquid. This can be illustrated by "air patterns" representing the distribution of molecules in the air. Actually the molecules are flying about at a high velocity across the vessel whose sides they are continually bombarding and therefore exerting pressure on them.

Calculation from the kinetic theory of gases shows that at 22° C. the oxygen molecules in the air are flying at a velocity of about 1600 ft. per second, the nitrogen molecules at about 1700 ft. per second. This velocity is not the mean but the square root of the mean square of the actual velocities of the particles. The molecules collide and zig-zag about in the enclosing vessel, so that it is only by imagination that we are able to conceive them as standing still and forming a pattern something like the pattern on a wall-paper.

When a spark is passed in a mixture of air and a hydro-carbon such as pentane a re-arrangement of the molecules takes place. The 5 atoms of carbon in the pentane molecule produce 5 molecules of carbon dioxide ; 12 atoms of hydrogen produce 6 molecules of steam. Before ignition there are 41 molecules, including 32 molecules of nitrogen. After the explosion there are 43 molecules, nitrogen taking no part in the change. Oxygen ceases to exist as a separate entity. The result is that every pound of pentane so transformed produces 10,000 lbs. calories of heat

The immediate effect of this production of heat is to increase the

velocity of the flying molecules. The actual velocity of the products of combustion in the vessel depends on the mean temperature. Direct measurement of the temperatures of the working charge of a gas engine gives 2570° abs. as a reasonable temperature from which to calculate molecular velocities. At this temperature the carbon dioxide molecules are moving at 3950 ft. per second, the steam molecules at 6166 ft. per second, and the nitrogen molecules at 4950 ft. per second, these numbers being the square roots of the mean squares of the actual velocities.

The next point for consideration is the time taken to effect this change. The time-interval taken by oxygen to combine with carbon and hydrogen lies along a time scale beginning with a detonation and ending with slow burning. In a mixture of air and pentane the oxygen molecules are a long way, on the average, from the carbon and hydrogen of the pentane molecule, and also the freedom of action of the oxygen molecules is clogged by the inert nitrogen present, but the rapidity with which oxygen can combine when the circumstances are favourable is shown by nitro-glycerine.

Chemists have discovered how to produce this nitro-glycerine molecule so that oxygen lies side by side with the carbon and the hydrogen. Its action is unclogged by any other substance, and the molecular distances have been annihilated, or perhaps it would be better to say that they have become atomic distances. Moreover, the molecule contains almost the exact quantity of carbon and hydrogen required to satisfy the oxygen present. As expressed by Lord Moulton, it is a case of the lion and the lamb lying down together. A mechanical shock causes an immediate transformation—the lion devours the lamb; and the time-interval for the meal is so short that it is not measurable. This is called a detonation. Chemists have shown by their researches how to combine nitro-glycerine with other substances in order to control the rate of combustion. Engineers are also trying to get control of the rate of combustion of some of the mixtures used in the internal combustion engine. Thus the chemist and the engineer are working in different parts of the same wide field of research.

Experiments initiated by Sir Dugald Clerk are now proceeding at the National Physical Laboratory under the general supervision of the Aeronautical Research Committee for the Air Ministry. Apparatus of the most refined nature has been devised, and the research is being carried out by Mr. Fenning. Various combustible mixtures are made up in a bomb. These are exploded and the time taken for the chemical combination to take place is recorded. Two results may be mentioned: a mixture of one part by volume of hydrogen, $2\frac{1}{2}$ parts by volume of air, was compressed to 64 lbs. per sq. inch and then exploded. Between the passage of the spark and the beginning of the rise of pressure about four-thousandths of a second elapsed. The combination was complete in about the same

interval of time. In another experiment the mixture was diluted with 1 part of hydrogen and 6 parts of air; this caused delay in the combination, which took six-hundredths of a second to complete. In such diluted mixtures the energy has to be shared by all the molecules which do not take part in the change.

The engineer is faced with two problems: the problem of a too rapid combustion, becoming a detonation, and the problem of a combustion too slow for complete combustion at high speeds.

In practice the turbulence and eddies caused by the rapid admission of a charge through the narrow annulus of an open admission valve results in quickening the rate of combustion, and it is owing to this cause that the gas engine can run at speeds greater than those corresponding to the measured rate of flame propagation for an efficient mixture. Sir Dugald Clerk found a striking difference in the area of indicator diagrams according to whether the mixture was exploded immediately after the admission valve was closed or whether it was exploded after precautions had been taken to damp out the eddies.

Among the problems arising from running internal combustion engines at high speeds is that of torsional oscillations, and synchronous oscillations. There is also the balancing problem. The four-cylinder petrol engine is usually constructed so that it is perfectly balanced for primary forces and couples, but gives the maximum error for unbalanced secondary forces. At certain speeds a model of this type suspended from springs will oscillate twice as fast as the speed of rotation of the engine, while at the same speed and on the same springs a model, balanced to eliminate the secondary forces, will run steadily at all speeds.

Other problems have also to be considered. Accurate records of the pressure-volume relation in the internal combustion engine must be obtained. and the difficulties are increased owing to the high speed at which the cycles take place. The direct measurement of temperature is also a difficult matter, and there are various fuel problems.

Sufficient has been said to show that the future of the internal combustion engine is not settled; it is full of problems requiring continuous and laborious research. We may well ask what provision has been made for this research. Before the war purely scientific research on the internal combustion engine was focussed largely in a Research Committee established by the British Association at the Dublin meeting in 1908. This Committee was the only one of its kind, and the work was carried on vigorously until the war under the successive distinguished chairmen, Sir William Preece and Sir Dugald Clerk. The Committee is still in existence. There is also the Research Laboratory at Shoreham under the direction of Mr. H. R. Ricardo, himself a distinguished scientific investigator.

During the war official organisations have been established, and now the Department of Scientific and Industrial Research provides

aid in money, apparatus, advice, and encouragement to any individual worker who has ideas and is qualified to carry on a research alone or under direction. This is a great national asset. But above all, so far as the petrol engine is concerned, there is a powerful organisation for Research within the Air Ministry itself, under the supervision of Air-Marshal Sir Geoffrey Salmond (known as the Director-General of Supply and Research), and under the immediate direction of Brigadier-General Bagnall-Wild, officially known as the Director of Research. The Air Ministry is advised by the Aeronautical Research Committee under the chairmanship of Sir Richard Glazebrook. This Committee has grown from the old Aeronautical Advisory Committee of the late Lord Raleigh. Work of the highest scientific value is now in progress at the National Physical Laboratory, at Farnborough, and at other places under the direction of the Ministry.

All I have done here is to hint at some of the work now going on at the National Physical Laboratory; it would take a whole evening merely to epitomise the researches in progress at that institution. Farnborough is now entirely a research establishment in its widest sense, for it is organised both for laboratory and for full-scale work. Work on the internal combustion engine has reached a magnitude and an intensity undreamt of before the war. The war has, in fact, shown that the internal combustion engine from being merely a convenient prime mover to put in our motor cars, to drive our workshops, or even our ships, has become an engine vital to our very existence. The Aeronautical Research Committee realises this, and so does the Air Ministry. Let us hope that the nation will realise it too, and that in the need and passion for economy our legislators will not starve research on this nationally vital prime mover.

[W. E. D.]

MARINE BIOLOGICAL LABORATORY
WOODS HOLE
LIBRARY
MASS.

WEEKLY EVENING MEETING,

Friday, June 2, 1922.

The Right Hon. Lord Justice Younger, P.C. G.B.E., Vice-President, in the Chair.

Hon. Maurice Baring.

Gilbert and Sullivan.

[With Musical Illustrations by Major Geoffrey Toye.]

The late Arthur Strong, who was librarian of the House of Lords, and not only a scholar of encyclopædic knowledge, but who also had a rare appreciation of all the arts, and an appreciation based on knowledge, used to say that the greatest English composer England had produced since the days of Purcell was Arthur Sullivan, the Sullivan of *Pinafore* and *Ruddigore*, and not the Sullivan of the *Golden Legend*, and that compared with him most of our modern composers were but the grammarians of music. He may have been right or wrong about modern composers; he may have been unjust; he was not speaking on oath. But it is certain that Sullivan carried on the true tradition of English music, or rather that in his work the English musical genius that produced tunes like "The Girl I Left Behind Me" and "The Bailiff's Daughter of Islington" was born again and flowered once more in a glorious spring-tide. The melodies in Sullivan's comic operas are as English as those older tunes, that is to say, as English as a picture of Constable, a lyric of Shakespeare, as English as eggs and bacon.

No foreigner, however painstaking, or however assimilative, can cook eggs and bacon, just as no Englishman can make French coffee. No nation can learn to make something which is peculiar to the genius of another nation. The most striking instance of this I can recall was the case of aeroplane manufacture during the war. When the French made English machines from English designs, and the English made French machines from French designs, the results were never satisfactory. A French designed machine made by Englishmen was never the same as a French machine, and an English designed machine made by a Frenchman was never quite like an English machine. And when the Germans copied either, the copy though accurate and faithful was Teutonic.

It is perhaps because Sullivan's lighter music is so essentially

English that it has taken years to obtain serious recognition. The tunes achieved instant popularity because they were English, but it was probably because of this instantaneous and widespread success that people failed to perceive the rarity and the value of the gifts which were being so freely bestowed upon them. They knew the tunes were catchy. They kept on humming them. They admitted them to be pretty; but they did not realise their inestimable, their unique artistic price. They felt as people feel when they see the work of a great water-colourist, or, indeed, of any great artist. "Oh, anyone could do that! We could do it ourselves if we knew how to paint or to compose." It seemed so simple, so easy. The essentially English quality of the stuff made them feel this all the more strongly.

The tunes seemed as easy to produce as the improvisations of a schoolboy playing with one finger. It was only when Sullivan was dead, and after many years of experience of the barren fruits of English musical comedy, that the public began to wonder whether after all the matter was quite as simple as they had thought. And when, after many years, there was two years ago a revival on a large scale, in London, of the greater number of the operas, many of us experienced a shock of surprise. The tunes were as catchy as ever, but the daintiness, the elegance, the finish, the workmanship, the beautiful businesslike quality of the work, its ease and distinction, its infinite variety, forced themselves upon the attention of everybody. The large public recognised at once that here was something which not everyone could do; and that nothing at all like it was being done, or had been done, by anyone else for years. The revival of *The Beggar's Opera* underlined the fact. That garden of English melody enhanced the authenticity of Sullivan's gift. It endorsed the credentials and the lineage of his music, and of his charm. It proved that he was no bastard and no pretender, but a rightful heir of Purcell, and a lawful representative of Merry England. What a joy it was, we all felt, when Gilbert and Sullivan and *The Beggar's Opera* were revived, to hear real English music once more! Not the slosh of ballad concerts, nor the jangle and rattle of ragtime and of modern revues, with their grating metallic tang and twang, their exasperating hesitations and their alien languor, but the music of the English soil; so noble, so gay, so debonair, so beautiful. The music that grew in England like wayside flowers, of which Purcell wove garlands, which the cavaliers put in their velvet hats, and the soldiers of the Georges wore as a cockade or flung to the girls they left behind them; flowers which were then neglected for many years, until Sullivan planted his rollicking border; flowers which were forgotten, buried under rubbish, and artificial and tawdry exotics, until the war at moments cleared those weeds away, and the soldiers in Flanders and France marched once more to the old rhythms, and invented preposterous but entirely English words to

the native airs of their country. Now it is extremely doubtful whether we should ever have been enriched with this precious legacy of English music if Sullivan had never met Gilbert. It is to this marvellously fortunate conjunction and collaboration that we owe this exuberant and entrancing revival of English dance, rhythm, and song.

It was Gilbert's rhythms, Gilbert's wit and fancy, Gilbert's fun and quaint mockery, Gilbert's whimsical poetry that played the part of the blue-paper packet of the composite Seidlitz powder, and when mingled with the white-paper packet of Sullivan's music produced the enchanting effervescing explosion. It is this which makes it impossible in talking of these operas to dissociate Gilbert from Sullivan, and to judge either, as far as the comic operas are concerned, separately.

The Gilbert of the operas has been compared to Aristophanes; and the comparison has been said to be a wild one. To place Gilbert in the same rank as Aristophanes, it is said, would mean he should have written lyrics as beautiful as those of Shakespeare. But to compare Gilbert and Sullivan with Aristophanes is not, I think, a wild comparison, for the lyrical beauty which is to be found in the choruses of the Greek poet, is supplied, and plentifully, by the music of Sullivan. I once heard Anatole France say that, speaking in an exaggerated way, the texts we possessed of the plays of Aeschylus were in reality librettos of operas of which the music was lost, as if, for instance, we only had an operatic libretto of *Hamlet* or *Faust*. If the Greek music was as good as the words we must have lost a good deal; but we can't tell. It has perished. Fortunately, Sullivan's music has not perished and Gilbert's text is complete. It does not for its purpose need to be any better. For its purpose not even Aristophanes could have improved on it, because the point about Gilbert's lyrics and Gilbert's verse is that it is just sufficiently neat, lyrical and poetical, besides being always cunningly incomparably rhythmical, to allow the composer to fill in the firm outline he has traced with surprising and appropriate colour.

Take these four lines of a trio from the First Act of *The Mikado* :—

> "To sit in solemn silence in a dull dark dock
> In a pestilential prison, with a life-long lock,
> Awaiting the sensation of a short, sharp shock
> From a cheap and chippy chopper on a big black block."

There is nothing very remarkable about this happy jingle, but Sullivan's handling of it makes one think of Bach.

If Gilbert had been a great verbal poet, a poet like Shelley or Swinburne, there would have been no room for the music; the words would have been complete in themselves: their subtle overtones and intangible suggestions would have been drowned by any music, how-

ever beautiful. As it is, the words have just enough suggestive beauty, and are always unerringly rhythmical, and this is just the combination needed to enable the composer to display his astonishing musical gift. I don't pretend to any musical knowledge whatever, but it is not necessary to be a trained musician to recognise and to feel the amazing powers of musical rhythmical invention which Sullivan displays throughout these operas. His rhythmical invention seems to be inexhaustible and infinitely various.

You have exquisitely funny and appropriate rhythm like his setting to Ruth's song in the First Act of *The Pirates of Penzance*:—

"When Frederic was a little lad he proved so brave and daring,
His father thought he'd 'prentice him to some career seafaring.
I was, alas! his nurserymaid, and so it fell to my lot
To take and bind the promising lad apprentice to a pilot.
A life not bad for a hardy lad, though surely not a high lot.
Though I'm a nurse, you might do worse, than make your boy a pilot.

"I was a stupid nurserymaid, on breakers always steering,
And I did not catch the word aright, through being hard of hearing;
Mistaking my instructions, which within my brain did gyrate,
I took and bound this promising boy apprentice to a pirate.
A sad mistake it was to make and doom him to a vile lot,
I bound him to a pirate—you—instead of to a pilot."

Or the lilt of the rollicking duet in *Ruddigore*, "Oh, happy the lily when kissed by the bee"; or, perhaps most surprising of all, the sad, endless tangle of the Lord Chancellor's nightmare in *Iolanthe*, as delirious as Tristan's fever:—

"When you're lying awake with a dismal headache and repose is tabooed with anxiety,"

with its transition at the end in which the notes seem to smell of dawn and dew:—

"But the darkness has passed,
And it's daylight at last,
And the night has been long,
Ditto, ditto, my song,
And thank goodness, they're both of them over!"

But one need hardly say that the most salient and supreme of Sullivan's gifts is that of *tune*; the gift of pouring out a stream of beautiful bubbling melodies. Most of these tunes are part of the permanent furniture and limbo of our minds. They are on the mouths of all and chiefly on the lips of the young. They rise in the heart and gather on the lips unbidden. Let those who are inclined to think Sullivan's melodies too facile listen on the gramophone to the duet in *Ruddigore*, "The old oak tree," or turn up the score of *Princess Ida* and play the quartette, "The world is but a broken toy," or "Free from his fetters grim" in *The Yeomen of*

the Guard. This is such a beautiful tune that the public, when Mr. Derek Oldham sang it during the recent revival, never even encored it. They were too greatly moved to do so, too satisfied hardly even to applaud.

Sullivan has another gift which is the hallmark of great art, the gift of discretion, of leading up to an effect in such a way that the effect when it comes seems as sudden as an April shower and yet as inevitable as a flower opening.

For instance, the way a famous song is led up to in *Pinafore* :—

> "I am an Englishman, behold me.
> He is an Englishman :
> For he himself has said it," etc.

Or more striking still, in *The Mikado*, the music that precedes the phrase :—

> "For he's going to marry Yum-Yum."

Gilbert's favourite opera is said to have been *The Yeomen of the Guard*, and certainly he never wrote more beautiful words than :—

> "Is life a boon?
> If so, it must befall
> That Death, whene'er he call,
> Must call too soon.
> Though fourscore years he give,
> Yet one would pray to live
> Another moon!
> What kind of plaint have I
> Who perish in July?
> I might have had to die,
> Perchance, in June!
>
> "Is life a thorn?
> Then count it not a whit!
> Man is well done with it;
> Soon as he's born
> He should all means essay
> To put the plague away;
> And I, war-worn,
> Poor captured fugitive,
> My life most gladly give—
> I might have had to live
> Another morn!"

And Sullivan never wrote anything more exquisite than the music to this, nor than the duet, "I have a song to sing, O," and the unaccompanied quartette, "Strange adventure," in the same opera. Bnt here both the poet and the composer enter into successful rivalry with other composers of the past. The lyric "Is life a boon?" might have come from an Elizabethan song-book; the duet, "I have a song to sing, O," from an Italian opera. I would like to give one instance of something which only Gilbert could have written

and only Sullivan could have composed. An instance of the kind is, I think, the quintette in the Second Act of the *Sorcerer* :—

> "I rejoice that it's decided
>   Happy now will be his life,
> For my father is provided
>   With a true and tender wife.
> She will tend him, nurse him, mend him,
>   Air his linen, dry his tears,
> Bless the thoughtful fates that send him
>   Such a wife to soothe his years."

No poet except Gilbert would ever have thought of the phrase, "Air his linen, dry his tears." No composer could have clothed the words more appropriately or more exquisitely. But it is, perhaps, in *Iolanthe* that Gilbert and Sullivan display, if not their highest, their most peculiar qualities. *Iolanthe* is, I think, the most Gilbertian of all the operas, and the music is peculiarly characteristic of Sullivan. Nobody but Gilbert could have imagined the Arcadian shepherd, who is half a fairy—a fairy down to the waist ; but his legs are mortal—and is engaged to a ward in Chancery ; the susceptible Lord Chancellor ; the chorus of peers ; the philosophical sentry who thinks of things that would astonish you, and the final departure of peers and fairies to fairyland :—

> "Up in the sky
>   Ever so high
> Pleasures come in endless series.
>   We will arrange
>   Happy exchange,
> House of Peers for House of Peris."

In this opera we are in the centre and capital of the cloud cuckoo-land of Gilbert's invention, the headquarters of his fantastic fairyland. That Gilbert lived in fairyland, or rather that he created a fairyland of his own, is a fact that is often overlooked. He is credited with the honours, the supreme honours, of topsy-turvydom, so that whenever anything peculiarly contrary to common sense happens in the public life or the Government of the country, we call it Gilbertian, but he is not as a rule credited with the glamour of magic. And yet that he possessed the secret key which unlocks the doors of that tantalising country is proved by the verdict of those who are the sole and only judges, namely, children. Children know that the land of *Ruddigore*, of *The Gondoliers*, of *The Mikado*, *Iolanthe*, and *Patience* is fairyland—the real thing. Only a few months ago I had the opportunity of comparing the opinions of some children who had been taken to see first *Jack and the Beanstalk* at the Hippodrome and then *Iolanthe*. Their verdict was that *Iolanthe* was a real pantomime, and that *Jack and the Beanstalk* in its modern shape, interlarded with political allusions and music-hall tags, was not.

In Gilbert's world the impossible is always happening. The Arcadian shepherd does marry the ward in Chancery. Private Willis, of the Grenadier Guards, does sprout little red wings, and the Fairy Queen sees to it that he is properly dressed. The pictures come down from their frames in *Ruddigore*, and the picture that hangs at the end of the gallery in a bad light, comes to life in obedience to Gilbert's inflexible and impossible logic, and marries his old love. Even in the operas where there are no actual fairies and no element of the supernatural, no pictures coming to life, no dapper salesman brewing love-philtres as in the *Sorcerer*; even in a plain satire such as *Patience*, we look at things through a coloured glass, or a glass that reveals hidden colours, such as that which the wizard gave to the Prince in the fairy tale, and through which, when he looked at the stars, he saw that they were many-coloured instead of all of them being white. They would be many-coloured looked at through such a glass, of course. And constantly throughout this opera we hear the horns of elfland faintly blowing, especially when the twenty lovesick maidens languish vocal in the valley, or when they lead Bunthorne like a heathen sacrifice with music and with fatal yokes of flowers to his (and to their) eternal ridicule.

Or, again, when the Gondoliers embark on board the Xebeque and set sail for the shores of Barataria :—

"Away we go
To a balmy isle,
Where the roses blow
All the winter while."

That is one of the most important factors in the power of Gilbert, who here again was able to find a purveyor of fairy music in Sullivan, and I think that *The Mikado* has, perhaps, more than all the other operas, the quality of a fairy tale, although there are no fairies in it.

Another important factor in Gilbert's work is the quality of his satire. Some people detest it. It affects them like bitter aloes. But it owes its enduring permanence, not to bitterness, for it is never really bitter, but to a certain breadth and force which has two cardinal merits. Firstly, that of being dramatic, of getting over the footlights, of appealing to the component parts of a large and mixed audience, so that the stalls will smile at one line and the gallery be convulsed at another, and all will be pleased ; and, secondly, of being general enough to apply to the taste and understanding of succeeding generations. Gilbert's satire, although directed at the phenomena of his own time, had a Molière-like quality of broad generalisation, which applied not only to the fashions and follies of one epoch, but to the eternal weaknesses of unchanging human nature.

So that when the First Lord in *Pinafore* sings :—

"Stick close to your desks and never go to sea,
And you may all be rulers of the Queen's Navee,"

or when Private Willis says that every boy and every girl that is born into the world alive is either a little Liberal or else a little Conservative, the words go quite as straight home to a modern audience as they did to the public which first heard them.

But although Gilbert's satire is not bitter, it is undeniable that it sometimes has an element not only of downrightness, but of harshness in it. It is not savage, like that of Juvenal or Swift, but it is not too squeamish for a knock-out blow. This may sometimes, and does sometimes, ruffle and jar upon the sensitive. But these easily ruffled persons should remember that Gilbert's harshness is an ingredient which is to found in all the great comic writers; in Aristophanes, in Cervantes, in Molière, and indeed in any comic writer whose work endures for more than one generation. It is a kind of salt which causes the soil of comedy to renew itself; and in Gilbert's case it arises from his formidable commonsense. He never took his paradoxes seriously as so many of his successors did. He is as sensible as Dr. Johnson, and sometimes as harsh. Gilbert has often been blamed for gibing at the old. It is true that his jokes on the subject of the loss of female looks are sometimes fierce and uncompromising. But they are mild indeed compared with those of Aristophanes, Horace, and Molière; and on closer inspection, we find it is not really at the old he is gibing, but at the old who pretend to be young; at Lady Jane's infatuation for Bunthorne; at Katisha's pursuit of Nanki Poo. Such things exist, and if they exist we must not be surprised if satirists laugh at them, and laugh loud. What is exceptional in Gilbert's satire is that he combined with this downright strong commonsense and almost brutal punching power a vein of whimsical nonsense and ethereal fancy which generally goes with more gentle and flexible temperaments.

The third cardinal quality of Gilbert's work is almost too obvious to dwell upon, namely, his wit, both in prose and in rhyme; his neat hitting of the nail on the head, his incomparable verbal felicity and dexterity; and the peculiar thing about Gilbert's verbal felicity is its conversational fluency. He uses the words, the phrases and the very accent and turn of ordinary everyday conversation and yet invests them with a sure, certain and infectious rhythm, the pattest of rhythm; and rhymes that are always inevitable, however fantastic and farfetched. For instance:—

> "When the coster's finished jumping on his mother,
> On his mother,
> He loves to lie a-basking in the sun,
> In the sun.
> Ah, take one consideration with another,
> With another,
> The policeman's lot is not a happy one,
> Happy one."

Or again :—

> "But when the breezes blow,
> I generally go below,
> And seek the seclusion that a cabin grants,
> And so do his sisters and his cousins and his aunts."

We find the same pat neatness in his prose. Take Ko-Ko's explanation to the Mikado :—

"When your Majesty says 'let a thing be done,' it's as good as done—practically it *is* done—because your Majesty's will is law. Your Majesty says, 'Kill a gentleman!' and a gentleman is told off to be killed. Consequently that gentleman is as good as dead—practically, he *is* dead—and if he is dead, why not say so?"

Another remarkable fact about Gilbert's satire is this: Just those subjects which, when he treated them, were thought to be the most local and ephemeral, have turned out, as treated by him, to be the most perennial and enduring. Take *Patience*, for instance. *Patience* was a satire on the æsthetic craze of the 'eighties. It was produced in 1881. It was aimed at the follies and exaggerations of the æsthetic school—the greenery-yallery, Grosvenor-gallery, foot-in-the grave, hollow-cheeked, long-necked and long-haired brood of devotees of blue china and peacocks' feathers and sunflowers, who were the imitators, the hangers-on and the parasites of a group of real artists and innovators, such as Whistler, Burne-Jones and Rossetti.

Punch started the campaign of ridicule, and Du Maurier's pictures of the adventures of Maudle and Postlethwaite towards the end of the 'seventies, are amongst the most entertaining and delightful of his drawings. *Patience* is said to have killed the phase; but outside the pages of *Punch* it is doubtful if æsthetes were really very plentiful, and *Patience* was based on the legend of a few, of a very few, people. But in writing this satire, Gilbert, if he magnified the follies of his contemporaries, hit the bull's-eye of a wider target. He struck the heart of artistic sham, so that his satire is appropriate to any time and any place.

Wherever there is real art there is always exaggerated imitation, and wherever there is real admiration there is false admiration, too. In Bunthorne and Grosvenor, Gilbert drew two types which sum up between them the whole gamut of artistic pretension and humbug. In every false world of art there is always a Bunthorne who has discovered that all is commonplace, and the burden of whose song is "Hollow, hollow, hollow." There is always, too, a Grosvenor, the apostle of simplicity, who is ready to write "a decalet, a pure and simple thing, a very daisy — a babe might understand it. To appreciate it, it is not necessary to think of anything at all." There is always a rapturous maiden ready to say "not supremely, perhaps, but oh so all but."

In the great flood of latter-day verse the School of Bunthorne still exists :—

> "Oh to be wafted away
> From this black Aceldama of sorrow,
> Where the dust of an earthy to-day
> Is the earth of a dusty to-morrow."

That is Bunthorne's "little thing of his own," called "Heart Foam."

I will not quote from a modern Bunthorne—that would be far too dangerous—but this is how the brilliant parodist of *Punch* who signs himself "Evoe" travesties the modern Bunthorne :—

> "Now while the sharp falsetto of the rain
> Shampoos the bleak and bistre square,
> And all seems lone and bare
> A crimson motive floats upon the breeze."

I think Bunthorne would have been proud to sign these lines. Grosvenor's poem began :—

> "Gentle Jane was as good as gold,
> She always did what she was told."

And this school of elaborate simplicity still has disciples. The twenty lovesick maidens are with us still. They read Freud and they paint cubes, and listen with rapture to the music of Skriabin, and the more unintelligible they find it the better they like it. This doesn't at all mean that the art they admire is really sham, any more than the art of Whistler and Rossetti was sham in the 'eighties ; but it means that every school of art has always had, and always will have, foolish disciples who imitate and exaggerate the faults of the master without being able to emulate his excellences.

But there always comes a moment in the world of make-believe, whether it is the world of the *Précieuses-Ridicules* or the world of Dadists, when the voice of commonsense will come breaking in, like the chorus of Gilbert's heavy dragoons. The entry of these dragoons in *Patience* is one of those effects which show Gilbert's sure instinct for stage effect, his consummate stage-craft, his profound knowledge of the theatre. The sudden crash of the brisk music of commonsense and its clash with the Della-Cruscan world of vaporous nonsense is not only comic but dramatic and *scenic*. It appeals to the eye as well as to ear and the mind. It is comic and dramatic by the contrast it makes, by the shock of surprise it gives, and the incongruous situation it creates ; and it is scenic by the picture it presents. The very uniforms conspire with their brilliance and unabashed primary colours to, as Henry James would say, "beautifully swear" with the Whistlerian and pre-Raphaelite colours and arrangements in pink and mauve and sage-green of the rapturous maidens.

To some people the chorus of those heavy dragoons will recall a

picture of an epoch that is as far away now as Nineveh and Tyre. The picture of London of the 'eighties; the bands playing "A magnet hung in a hardware shop" in the streets in the morning; the Park in the afternoon, crowded with elegant carriages, barouches, and victorias, a highly-perched dowager waving a small gloved hand; Rotten Row in the morning, crowded with top-hatted cavaliers and ladies witching the world with horsemanship and faultless habits; the photographs of Mrs. Langtry and the professional beauties in shop windows; the perfumed, padded, silken missives of St. Valentine's Day; the little flat bonnets with bows; the Du Maurier ladies, haggard from adoration, green with love and indigestion, at the classical concerts; and the Princess of Wales driving past in an open carriage as beautiful and as graceful as Queen Alexandra. And before leaving the subject of *Patience*, I should like to end with one quotation which contains, I think, the whole essence of Gilbert and Sullivan, so that if this song alone survived we should know what was the best they could do, both of them:—

"Prithee, pretty maiden, will you marry me?
Hey, but I'm hopeful, willow willow waley.
I may say at once I'm a man of propertee,
Hey willow waly O.
Money I despise it,
Other people prize it,
Hey willow waley O."

Gilbert never wrote anything better than that, and Sullivan, as usual, rose to the occasion, and clothed these tripping syllables with a most delicate vesture of melody, in which a fairy-like pizzicato accompaniment falls on the thread of tune, like dewdrops on gossamer. If this song had had German or Italian words, and had reached us from Vienna or Milan, the critics would have made as much fuss over it as over any tune in Mozart.

Cannot you imagine it being warbled by an Italian welter-weight prima donna and a luscious Italian tenor?

"Non del mio amore Donna ti scordar,
Deh! esperanza, sorgi in cuore mio,
Dai miel soldi non c'èda dubitar
O salice senza un Addio!"

Or in German something like this:—

"Willst Du, hübscher Jungfer, nicht mein Weibchen sein
Bin Ich doch hoffnungsvoll, O Weide Wehe,
Will es Dir gleich sagen Hab' ein Schloss am Rhein
O Weide Wehe."

Or in French:—

"Charmante bergère, je demande ta main!
(Tremble mon cœur comme un saule pleureur!)
Sache sans mystère, je possède un moulin.
(Oh la joie, la joie fait peur.)"

Or words to that effect. I don't pretend that they are correct. That tune, when *Patience* was first produced, was whistled in the streets and taken for granted as one of the popular airs of the day; but how few people at the time recognised its rarity as a gem.

You have only to look at the back numbers of *Punch* to see how niggardly critical opinion of all shades was of its praise of these masterpieces when they were first produced. And I remember myself hearing grown-up people talking of them as if they were so much scaffolding for the display of the actors of the day, who, we must not forget, were then, as they still are now, quite exceptionally remarkable.

It is seldom that one cast included two such exceptional artists as George Grossmith and the great baritone who has just left us, Rutland Barrington. They did more than perfectly fill their parts. They inspired Gilbert and Sullivan to create new characters: Grossmith with his perfectly natural fantasy, and Barrington with his suave imperturbable gravity.

It must be a comforting thought for modern musicians to think that it takes about thirty years for people to appreciate their music at its true value, even when, as not always happens, it wins instantaneous popularity. But when *Princess Ida* was first produced the verdict of *Punch* and of the public was: "No Grossmith part," just as they now might say: "No Leslie Henson or no Nelson Keys part."

Sometimes, as in the case of Bizet, a masterpiece, and what was to prove one of the most popular of operas, namely, *Carmen*, was kept for years, unacted, in the drawer of a manager.

I remember once during Holy Week at Moscow, when there was a fair going on at the Kremlin, seeing a little old man hawking about some gold-fish in a very small bottle.

He kept on piping out in a high falsetto:—

> "Fish, fish, fish, fish, little gold-fish,
> Who will buy?"

"Who will buy?" he piped, as he walked up and down between the bookstalls and the booths. But the people bought toys and sugarplums, cloths and books, boots and old odd volumes of *Punch* and John Stuart Mill and Mrs. Humphry Ward—but no gold-fish.

No one would buy the little gold-fish; for men do not recognise the gifts of Heaven, the magical gifts, when they see them. In the case of Gilbert and Sullivan they bought at once; but they thought that the gold-fish were as common as dirt. It was only when the sellers were dead that they recognised that what they had been buying so easily and so cheaply was magical merchandise from fairyland; that there was nothing to match it and nobody else to provide anything of that kind any more.

Even now, it is doubtful whether Sullivan's music has received the serious recognition it deserves. Critical people, the serious that

is to say, are always prone to despise a gold-fish because it is gold and looks pretty, and they are sometimes inclined to patronise tunes if they are gay, light and joyous. Anything in art that is ponderous, serious, complicated and unintelligible is at once respected; but if a tune is gay and easy, a poem rhythmical and well rhymed, a picture pleasantly coloured, with a subject that is perfectly plain, so that if it represents a field, the field looks like a field, and not like the forty-second proposition of Euclid, the serious are inclined to look at it askance. I remember in 1914 some academicals wrote indignantly to the newspapers, because "Tipperary" was a popular tune, and this roused Dr. Ethel Smyth, a judge of tune if ever there was one, to wrath; and she wrote to say she was certain that the tune of "Tipperary" would have delighted Schubert.

Some people will never forgive Sullivan for being popular, and never admit that a tune which can be as infectious as small-pox in a slum should be taken seriously. But the whole point of really great art is that while it satisfies the critical it pleases the crowd, that while children can enjoy it, it fills the accomplished craftsman with despair at being unable to emulate it: Bunyan's *Pilgrim's Progress*, *Alice in Wonderland*, Gray's *Elegy*, and *The Midsummer Night's Dream* are instances in point.

But there is no reason to be despondent. Gilbert and Sullivan's operas, always popular, are now receiving the best kind of recognition, although there are still some dissentient voices and still some implacable high-brows. And they are as popular with the young generation as they were with the old. About this there is no possible doubt whatever; when they are given at the Universities now, they are even more popular than lectures on relativity, and the undergraduates crowd to them. About their popularity in London there can be little doubt when people are ready to sit outside the theatre for twenty-four hours to be present at the last performance of the season.

At the Prince's Theatre during the recent admirable revival of the operas, there was something in the atmosphere of the theatre which was different from that at all other theatres in London, except the "Old Vic." You felt at once you were forming part of an audience that definitely knew what they liked. They were there to enjoy themselves, and they knew they *would* enjoy themselves. This in itself is to some people unpardonable.

The operas were enjoyed by the old who saw them through mists of many memories, and who were not disappointed with their present-day interpretation. They were enjoyed by the young, and they came as a revelation to those who had never seen them before. Children found in them the most magical of pantomimes; politicians, the keenest and the most actual of satires; musicians, a treasure-house of skill and invention; writers and playwrights, an ideal of verbal felicity and stage-craftsmanship, far beyond their reach.

One night, during the recent revival of *Iolanthe,* I was sitting next to a celebrated modern author and an extremely accomplished manipulator of words. When the chorus sang :—

> "To say she is his mother is a bit of utter folly !
> Oh, fie ! Strephon is a rogue !
> Perhaps his brain is addled and it's very melancholy !
> Taradiddle, taradiddle, tol lol lay ! "

he said to me, "That's what I call poetry," and he added that he thought that the most permanent and enduring achievement of the Victorian age would be neither that of Tennyson, Browning or Swinburne, or Gladstone, Disraeli and Parnell, or Darwin, Huxley and Ball, but the operas of Gilbert and Sullivan. I am inclined to agree with him ; and I should not be in the least surprised if, in ages to come, people will talk of the age of Gilbert and Sullivan, as they talk of the age of Pericles. Perhaps they will confuse fact with fiction, and the children of the future will think that trials by jury in that amusing age were conducted to music ; that pirates and policemen hob-nobbed at Penzance ; that Strephon, the Arcadian Shepherd, brought about the reform of the House of Lords ; that the Bolshevik Revolution took place in Barataria ; and the Suffragist movement happened at Castle Adamant.

In thinking of the triumph, and the permanent popularity, of these operas and the excellent manner in which they are produced and interpreted at the present day, it is impossible not to regret that we should only be able to hear them during a short season at intervals of two years.

What we want is a permanent Opera House, where not only Gilbert and Sullivan, but all other English music, such as *The Beggar's Opera*, and foreign music too, should be done all the year round.

What a grand opportunity is here for a model millionaire such as Gilbert would have invented, to create a permanent Gilbert and Sullivan House, at which other operas might be acted, new operas produced, and old operas revived ! Perhaps such a man will turn up one day ; for although all millionaires are not model, some of them are musical.

[M. B.]

WEEKLY EVENING MEETING,

Friday, June 9, 1922.

Sir James Crichton-Browne, J.P. M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

Joseph Barcroft, C.B.E. F.R.S.

Physiological Effects at High Altitudes in Peru.

The recent expedition to Peru was initiated under the auspices of the Royal Society. So far as the British members were concerned, it was financed in part by a grant made by that body, in part by two substantial private subscriptions from Sir Robert Hadfield, then on the Council of the Royal Society, and Sir Peter Mackie, who has on previous occasions been a staunch supporter of anthropological research undertaken by the Royal Society. In part also its expenses were met by grants from the Moray and Carnegie funds in Edinburgh. These grants paid some of the expenses of the expedition as a whole, together with the personal expenses of three of its members—namely, Dr. J. C. Meakins, professor of therapeutics in Edinburgh; Mr. J. H. Doggart, of King's College, Cambridge; and myself. The project was warmly supported by a number of institutions on the American continent, each of which sent a member of the party at its own expense. Harvard Medical School was represented by Dr. Bock, Dr. Forbes, and jointly with Toronto Medical School by Prof. Redfield; the Presbyterian Hospital, New York, by Dr. George Harrop; and the Rockefeller Institute by Dr. Carl Binger. The American and British parties sailed from New York and Liverpool respectively in the middle of November, the American section arriving in Peru a fortnight or more before we did.

I have perhaps given the impression that the party consisted of two sections from different continents, sharply marked off from one another, and neither of which had seen the other before. This impression is erroneous, for the whole idea of the expedition grew from the fertile soil of collaboration in the researches carried out under a single roof. Dr. Redfield and Dr. Bock had been working in Cambridge (England) throughout the previous year, and Dr. Harrop had been there for a short time. There the scheme had been hatched, the methods standardised, and a number of the controls carried out.

Why did we go to Peru, or, more precisely, to Cerro de Pasco? The question may most easily be answered by comparing Peru with

some of the other localities to which we might have gone, and to which others have gone before us; for example, Monte Rosa, Pike's Peak in Colorado, the Peak of Teneriffe, and the Himalayas. Without going at length into the merits of each, the advantages of Peru will be sufficiently apparent if I compare it to one of the above, and I will select one of which I have personal experience, namely, the Peak of Teneriffe. Peru and Teneriffe have in common the merit of being close to the sea. In either case the baggage can be put on board at Liverpool or Southampton and taken to your mountain base without further transhipment. Peru, however, possesses the first necessity of laboratory equipment—an abundant supply of water—up to a height of 16,000 ft., i.e. 4000 ft. higher than the Peak. In the latter place the highest altitude at which I know of water is 7000 ft., while at 11,000 ft.—near the situation of the Alta Vista Hut—there is an ice-cave from which water may be obtained by melting the ice.

Again, the conditions of transport are vastly different in Peru from what they are in Teneriffe. In Teneriffe everything goes up the mountains by mule. The amount of apparatus which can be taken up is therefore small; and if it arrives whole at its destination the worker is fortunate. If it arrives broken, there is little hope of mending it. We were very fortunate, at an early stage of our preparations, in getting in touch with Mr. Oliver Bury, the Chairman of the Peruvian Corporation. The Peruvian Corporation owns, among other railways, the trunk line which goes directly inland from Lima, climbs the Andes to a height of almost 16,000 ft., and then drops down to Oroya (12,000 ft.), situated on the pampa between the two parallel ranges of the Cordilleras. From Oroya railways run north to Huancayo, and south to Cerro de Pasco (14,200 ft.), which place was to become our principal seat of operations. To the Peruvian Corporation we owe our laboratory. For the purpose we were assigned a luggage van, 45 ft. in length, together with a goods van which we used as a store: and these they offered to take to any locality on their system at which we desired to work. While the American members of the party awaited our arrival at Lima, they fitted up the luggage van and made a very fine laboratory of it. At one side the door was closed up and windows put in its place, benches and shelves were fitted, electrical wiring was installed, and ultimately we had electric light, power and heat. What greater contrast in efficiency could exist between our mobile laboratory at Cerro, jacked up off the bogies to prevent vibration, fitted with X-ray plant and apparatus for the measurements of hydrogen ions, on one hand, and the Alta Vista Hut in Teneriffe, with its paraffin stoves which emitted little but smuts and barely sufficed to melt a few handfuls of ice? Of more account, however, than all these advantages was the fact that, up to an altitude of 16,000 ft., in Peru there is a population most of which is connected with the mining industry. This popu-

lation may be divided into two categories, namely, the Anglo-Saxon officials and the native labourers. The latter are of Indian descent, and as a race have lived at this altitude for many generations. In Cerro they are designated "Cholo," a name that has no exact anthropological significance, but I shall use it and so avoid an assumption of anthropological knowledge which I do not possess.

To judge from the customs which prevail in the outlying villages, the Cholo is not far removed from a very primitive civilisation. Within a mule-ride of Gollarisquisga there are communities in which private ownership of land does not exist; the land, as in some of the Russian communities which are, or were, on the Canadian prairie, belongs to the village. The produce, if the village is small, is placed in the church; in the larger villages there is a store for this purpose. If the stock of some community has run out, some person goes to such a market as Huancayo and buys some more, not for himself, but for the village. I said "buys"; but there are places to which money has scarcely penetrated, and where the exchange of commodities is still a process of barter. The condition of medical science in these villages may be gathered from the fact that such nostrums as horse-dung and well-kept human urine occupy an honourable place in the pharmacopœia, and that a custom appears to linger by which, when the practitioner has done his best—or worst—and failed, the services of another official are called in. He is the "despenador," or "putter out of pain." I need say no more of his or her duties than to give the following quotation from Bensley's "Spanish and English Dictionary": "Despenadora—a woman who is supposed to push her elbow into the stomach or breast of dying persons to relieve them from agony." *

It seems clear, then, that the Cholo, not the Cholo of Cerro de Pasco or Oroya, but some of the far outlying districts, has been little touched by the Spanish or even the Inca civilisation, and that in him we have a subject for physiological research whose like has varied little for generations. In physique he is short in stature and sallow, or with some blood in his cheeks. That part of his anatomy which was principally of interest to our party was his chest. We made a considerable number of chest measurements. As regards the chest circumference the following statement sums up our findings. We based our measurements on Prof. Dreyer's tables, accepting his estimate of the normal ratio between the trunk length and the chest circumference. We ascertained that the average circumference of the Cholo chests which we measured would normally be 79 cm. It was, in point of fact, 92 cm. As a rough check we measured our own trunk chest ratios and those of the American and British engineers, a community of fine physical development. The circum-

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* I am indebted for this quotation and much else to Mr. Murdock, manager of the coal mine at Quishuarcancha.

ference of the Anglo-Saxons was little in excess of Dreyer's estimate. The lowest level at which we came across one of these small people with chests which appeared out of proportion to the rest of his stature was at Matucana (8000 ft.), and on inquiry we found that he was a native of Huancayo (12,000 ft.).

To pass to the more strictly physiological aspects of the work of the expedition, one must reflect that the desire to investigate mountain sickness goes back at least to the middle of the last century. It is remarkable, when one comes to think of it, how recently our knowledge of the causation of disease has grown. The lure of mountain sickness to the physiologist lay originally in the fact that it was a disease to which a definite cause could be assigned. You go a certain height up the mountain—any mountain—and when your ascent corresponds to a given fall in the barometer you suffer from mountain sickness; when you descend, the malady leaves you. Mountain sickness, or, as it is called in Peru, "seroche," seemed to form a sort of opening into the ætiology of disease.

In recent years the centre of interest has to some extent shifted. The cause of mountain sickness is universally regarded as insufficient oxygen supply to the tissues of the body, though there may still be some doubt as to the directness of the connection between the deficiency of oxygen pressure in the blood and the activity of the nerve cells responsible for the continence of food in the stomach. Interest latterly has centred rather around the methods which the body has at its disposal for adapting itself to such a condition. But the same thread still runs through the fabric; this particular instance of adaptation to environment is studied because our knowledge of the conditions with which the body has to deal are so exact and the conditions themselves so easily produced or abolished.

Partly, of course, it has another interest, namely, that imperfect oxygenation of the blood is a factor in a number of pulmonary complaints, and an analysis of those complaints demands an investigation of this particular factor. That is the definitely medical aspect, of which I shall say but little. Rather I shall turn my attention to the extent to which adaptation can take place, and the means by which it is brought about.

Some of the Cholos appear at first sight to have acquired an astonishing capacity for physical effort at high altitudes. An example may be cited. Near Cerro de Pasco there is a mine worked in the old Spanish way. The ore is raised from the bowels of the earth on the backs of porters, who carry their loads up a rude staircase. The mine is about 250 ft. below the surface, and the staircase about 650 ft. in length. It opens under a sort of hut. The first porter whom we saw emerge was a little fellow, who said that he was ten years old. We so far doubted his word as to place his age at thirteen or fourteen years. He had on his back a load of ore which I estimated at 40 lbs.—and that at an altitude at which the barometer

stood at only 450 mm., or about 18 in. Shortly a more mature boy appeared—perhaps seventeen or eighteen years of age—his load was about 100 lbs. To understand these feats, it must be remembered that exercise may be of two kinds, spasmodic or continuous. In the case of continuous exercise, such as that of long-distance running, the subject must maintain an approximate equilibrium between the oxygen which he uses and that which he absorbs. His oxygen account must, so to speak, balance approximately at any given time. In the case of spasmodic exercise it is otherwise. If the subject is prepared for the exercise to cease after a very short time, he may expend oxygen at a greater rate than he takes it in, and thus overdraw his oxygen account. A limit is, however, set to the overdraft, and when that limit is reached he must rest till his oxygen account has righted itself. This formed the subject of a most interesting investigation carried out by Dr. Lupton recently in the laboratory of Prof. A. V. Hill. The porters in the old Spanish mine raise their loads by a series of spasmodic efforts, each of which is followed by a rest of considerable length accompanied by great respiratory distress.

Of the means by which the body acclimatises itself to oxygen want, real or alleged, we investigated the following :—

1. *Secretion of Oxygen by the Pulmonary Epithelium.*—Numerous direct estimations were made of the oxygen pressure in the arterial blood, and in the alveolar air. The two usually came out within two or three millimetres of one another, which is approximately the experimental error of the method. Such a coincidence can only mean that the oxygen passes into the blood by a process of diffusion through the very attenuated partition of epithelium which separates the air from the blood in the lung. Thus the view that the lung could enable the body gradually to overcome the effects of altitude by creating a sort of forced draught and maintaining the oxygen pressure in the blood at its sea-level value is unfounded.

However, the blood as it leaves the lung must contain appreciably less oxygen than its hæmoglobin would normally absorb. It is, to use the American phrase, *un*saturated to a considerable degree. Such blood, of course, would lack the bright scarlet colour of true arterial blood. The actual colour of the blood as withdrawn from the radial artery entirely bore out this view ; as it flowed into the syringe it was of a dull red colour, often verging on chocolate, and in the case of the natives was 82–86 per cent. saturated with oxygen, instead of 95–96 per cent. as at the sea level.

Curves giving the relation between the percentage saturation of the blood and the partial pressure of oxygen in lungs at Lima and Cerro de Pasco for different members of the party are shown in Fig. 1, from which it is apparent that at high altitudes the partial pressure required to secure a percentage saturation sufficient for life decreases considerably.

The establishmeunt of the fact that life can be supported with

some degree of efficiency with the blood in this condition is of great importance, because in recent years there has been a tendency to assume that a small degree of unsaturation of the arterial blood must of necessity produce very grave results. Fig. 1 shows that there is some adjustment of the blood to the new conditions. At Cerro the unsaturation of the blood was written on the faces of the inhabitants. Anyone who had any colour in his face was appreciably cyanosed.

2. *Increased pulmonary ventilation* has been shown by all recent observers to be of great importance as a factor in adaptation to high altitudes. In our case, had our respiration been the same in rate

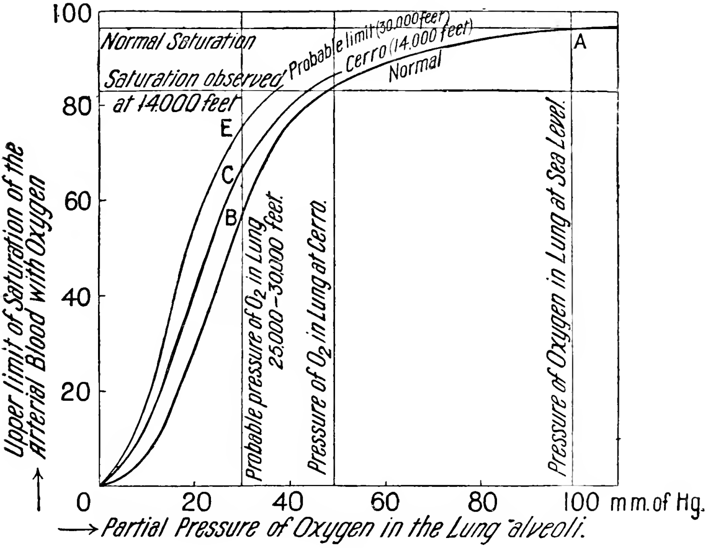

Fig. 1.

and depth at Cerro as it was at Lima we would have had about 40 mm. pressure of carbonic acid and 35 mm. pressure of oxygen in the air of our lungs. In fact, owing to increased respiratory effort, we reduced the carbonic acid to about 25 mm. and raised the oxygen to about 52 mm. The importance of these facts is enhanced by the certainty that it is the partial pressure of oxygen in the alveolar air which regulates the degree of saturation of the blood.

While the increased ventilation of the lung had been demonstrated by previous observers, the mechanism which was responsible for it had been much in dispute. This we investigated. The mechanism of hypernœa *at rest* seems to be that first suggested by

Haldane, namely, that the want of oxygen heightens the activity respiratory of the respiratory centre, resulting in a mild degree of forced respiration—so mild as not to be apparent to the subject, yet sufficient to reduce the carbon dioxide content of his blood. Incidentally this process acting alone would make the blood more alkaline. The measurements of hydrogen ion concentration in the blood of persons at rest bore out this view; either the blood was more alkaline than at sea level, or it was of approximately the same reaction.

The effect of exercise on the blood has been more fully investigated, though for the most part by indirect methods. Our results support those already obtained, namely, that a given increment in the hydrogen ion concentration of the blood is produced by less exercise at high altitudes than at the sea level. Thus the dyspnœa of exercise is the cumulative effect of two factors—an increased response of the respiratory centre to a given stimulus, and an increase in the stimulus evoking the response.

3. I have already alluded to the size of the Cholo's chest. With it appears to be associated an interesting modification of its configuration. Fig. 2 shows two X-ray photos of the left sides of two chests photographed from behind. Both pictures were made at Cerro de Pasco. That on the right is my own, and is fairly typical of our party; that on the left is a typical Cholo chest. There is a marked difference in the angle at which the ribs are carried; my own slope down from the vertebral column at a quite considerable angle, while those of the native are much more horizontal. It seems highly probable that this horizontal carriage of the ribs indicates a compensatory effort designed to increase the facility with which the blood obtains oxygen, for it is acquired at sea level by persons suffering from emphysema and other complaints in which there is shortness of breath. Several of the mining engineers, of whose chests we took radiograms, showed a similar tendency. At this point another peculiar physical conformation may be mentioned, namely, clubbing of the fingers, which, when found at sea level, is frequently associated with some trouble which prevents sufficient oxygen from reaching the extremities. Though they are not the rule, clubbed fingers are by no means unusual at Cerro de Pasco in persons without any circulatory or respiratory lesion.

4. An increase in the number of red blood corpuscles in each cubic mm. of blood has long been known to occur at high altitudes. Systematic researches carried out principally under the direction of Dr. Haldane have shown that the increase in the number of red blood corpuscles is associated with an increase in the quantity of hæmoglobin present. These two observations we have verified, and to them have added a third, namely, that the chemical conditions under which the hæmoglobin is held in the red blood corpuscle confer on it the peculiarly useful property of acquiring

more oxygen when exposed to rare atmospheres than is the case with normal blood.

5. We sought in vain for any such form of acclimatisation as might be afforded by the driving of an increased volume of blood round the body in unit time. A rather natural supposition would be that, if the hæmoglobin of each cubic centimetre of blood were deficient in oxygen, the tissues might be fed with sufficient oxygen by the simple process of giving them more blood: but this is not so.

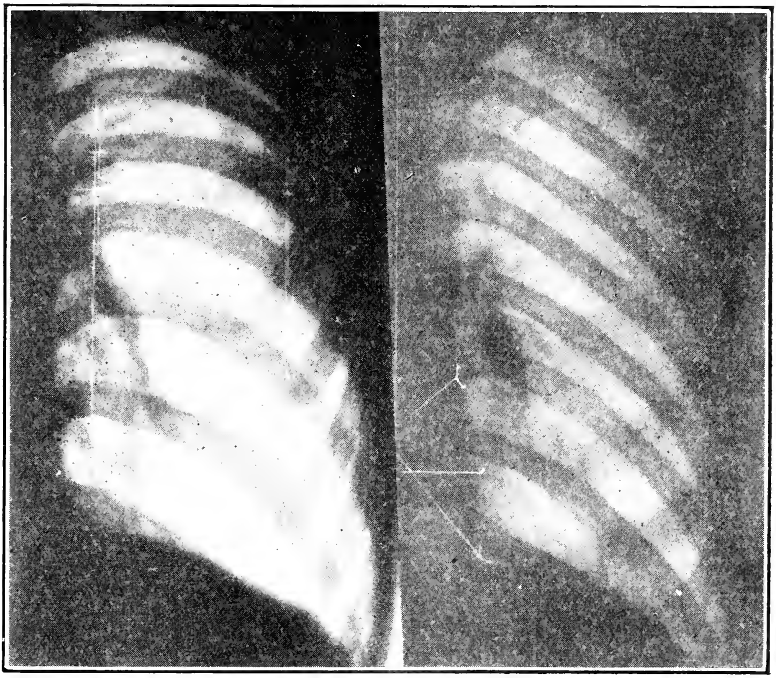

Fig. 2.

It is true the heart quickened with exercise, but the quickening seems to have been rather a signal of distress that a compensatory mechanism.

Three principal forms of compensation have been described: they are increased total ventilation, increased expansion of the chest, increased hæmoglobin, and increased affinity of the blood for oxygen: their relative importance is a matter for future research. Jointly or severally they may mitigate the effects of oxygen want, but they cannot entirely abolish them; at some altitude the human frame

must always succumb. We were naturally somewhat interested in the question of whether we could foretell which of our own party would succumb most quickly, and various members of the party worked out systems of prophecy which differed not only in character but in the prophetic order in which the various individuals would prove susceptible to altitude. One of these proved quite successful. It was based on the determination of Bohr's diffusion constant (the ratio of the quantity of oxygen absorbed per minute to the average difference of pressure between the oxygen in the alveolar spaces and alveolar capillaries) for the lung, and was suggested by Prof. Krogh. The members of the expedition could be divided into two distinct groups—those who had a constant for oxygen of more than 40 and those who had a constant less than that figure. One group with the higher diffusion constant suffered from obvious symptoms of mountain sickness, while the other did not. It is true that of the four who suffered the salient feature was different in each case; in one it was faintness, in another vomiting, in a third high temperature and intense headache, and in the fourth deafness and indistinct vision. Only further research can show whether the coincidence was fortuitous, or whether any causal relation exists between the diffusion constant and the tendency to "seroche." The hint, however, seemed to be worth taking, and in consequence an arrangement has been come to by which persons intending to go to the mining districts in the Andes are being tested in the Rockefeller Institute in New York.

I must also make some allusion to the goodwill which was extended to us by everyone with whom we came in contact in Peru, from the President down to the humblest employee of the Cerro de Pasco Copper Corporation. Of the manager and the officials of this company we can only say that their kindness in placing themselves and their resources at our disposal was one of the most potent factors in enabling us to achieve such scientific results as we obtained. No less can be said of the officials of the Pacific Steam Navigation Company.

The problem of Everest from the point of view of physiology, upon which our work in the Andes throws some light, may be stated thus:—

Every cubic centimetre of arterial blood which leaves the lung must contain a certain quantity of oxygen, expressed as a percentage of the maximum which the blood can hold, if life is to be maintained at a level consistent with any degree of efficiency. It is not known what this quantity of oxygen may be. The following considerations, however, give some clue to it:—

(*a*) Let the maximum quantity of oxygen (shown on the ordinate of the graph in Fig. 1) which the blood can hold be called 100.

(*b*) There is a certain relation in the blood for normal persons between the amount of oxygen it can hold and the pressure of oxygen to which it is exposed; that relation is shown in the graph labelled

"normal." (The partial pressure of oxygen forms the abscissa.) At the sea level the oxygen pressure in the lung is about 100 mm. and the quantity of oxygen in the blood 96 per cent. of the possible load. (See the point A on the graph.)

(*c*) Until recently it was supposed that the curve did not alter, and therefore the graph labelled "Normal" stood for all altitudes.

(*d*) Also the most competent authorities regarded an oxygen load of about 90 per cent. of the possible maximum as being required for the conduct of life—apart from short exposures.

(*e*) The probable partial pressure of oxygen in the lungs at 25,000–30,000 ft. is calculated by a process of exterpolation to be about 30 mm. Combining *c*, *d* and *e* above, the quantity of oxygen in the arterial blood on Everest would be 58 per cent. of the maximum—far below that necessary.

(*f*) The recent expedition to Cerro de Pasco has brought out two new points :—

(1) That natives lead a reasonably normal existence with blood charged only up to 82 per cent. of the possible, and Europeans with 85 per cent. of the possible, load of oxygen.

(2) That the graph changes in position, and for natives and Anglo-Saxons approximates to that labelled Cerro (14.000).

(*g*) On this graph a partial pressure of 70 mm. of oxygen in the lung might saturate the blood up to 67 per cent. (*c*).

(*h*) It is scarcely likely that the curve moves further than that marked "Probable limit (30,000 ft.)." On that curve, however, the blood would be charged up to 76 per cent.—a figure within a reasonable distance of what has actually been observed in the Andes.

[J. B.]

GENERAL MONTHLY MEETING,

Monday, June 12, 1922.

Sir James Crichton-Browne, M.D. LL.D. J.P. F.R.S.,
Treasurer and Vice-President, in the Chair.

Henry Cooke,
Eric Davies,
Miss Joan Evans,
Mrs. Agnes Jacobs-Larkcom,

were elected Members.

The Chairman announced the decease on May 26, 1922, of Ernest Solvay, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution of Great Britain deplore the great loss the Institution has sustained by the death of its Honorary Member, Ernest Solvay, Minister of State of Belgium; Member of the Academy of Sciences, Paris; Grand Officer of the Legion of Honour, Grand Cordon of the Order of Leopold; Honorary Member, American, German, French and Dutch Chemical Societies; Corresponding Member, Prussian Academy of Sciences; Lavoisier Medallist, Institute of France; Grand Medallist, University of Paris; Author of "La Gravito-Materialitique" (1911), and other Papers.

Ernest Solvay was one of the greatest inventors of the century, and by his ingenuity and enterprise became the founder of the Ammonia Soda Industry. He revolutionised the Basic Industry of the production of Carbonate of Soda from Common Salt by the use of Ammonia in the chemical reaction. The Solvay Process superseded the Leblanc Process, which involved the use of furnace temperature, by one acting at ordinary temperature, and therefore highly economical.

The inventions of Ernest Solvay enabled the late Dr. Ludwig Mond to establish the Ammonia Soda Process in this country as early as 1873; and subsequently Solvay Factories have been extended throughout the world.

He was a generous benefactor in many branches of pure Science, including Physiology, Physics, Chemistry, Public Health, Social Welfare. In 1893 he established two Institutes in Brussels, the University Institute of Physiology, and an Institute for Electro-Biological Researches.

He organised numerous International Conferences in Brussels to stimulate Scientific Research. In 1912 he established an International Institute of Physics, and in 1922 a similar International Institute of Chemistry, with specific endowments of one million francs to be expended in thirty years from their Foundation for the Promotion and Extension of Physics and Chemistry.

At the Centenary Celebration of the Royal Institution in 1899, Ernest Solvay attended and received the Diploma of Honorary Membership from the hands of the late King Edward VII., when Vice-Patron.

The Managers desire to express on behalf of the Members, their sincere sympathy with Madame Solvay and the family in their bereavement.

The Special Thanks of the Members were returned to Thomas W. Dewar, M.D. M.R.I., for his Donation of Twenty-Five Pounds to the Fund for the Promotion of Experimental Research.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Nasmyth, J. G., Esq., J.P. M.D. D.Sc. (the Author)—The Kingdom: Its Characteristics and Distinguished Sons. 8vo. 1922.
New York, Society for Experimental Biology—Proceedings, Vol. XIX. Nos. 5–6. 8vo. 1922.
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Onnes, Dr. H. Kamerlingh, Hon. M.R.I.—Communications from the Physical Laboratory of the University of Leiden, No. 156. 8vo. 1922.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, April 1922. 8vo.
Paris, Société Française de Physique—Journal de Physique et le Radium, Série VI. Tome III. No. 4. 8vo. 1922.
Peru, Corps of Mining Engineers—Bulletin Nos. 102–103. 8vo. 1921.
Pharmaceutical Society of Great Britain—Journal, May 1922. 8vo.

Photographic Society, Royal—Journal, N.S., Vol. XLVI. No. 6. 8vo. 1922.
Physical Society—Proceedings, Vol. XXXIV. Part 3. 8vo. 1922.
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Royal Engineers' Institute—Journal, Vol. XXXV. No. 6. 8vo. 1922.
Royal Society of Arts—Journal, May 1922. 8vo.
Royal Society of London—Proceedings, A, Vol. CI. No. 709; B, Vol. XCIII. No. 653. 8vo. 1922.
Philosophical Transactions, A, Vol. CCXXII. Nos. 601–604; B, Vol. CCXI. Nos. 385–387. 4to. 1922.
Sanitary Institute, Royal—Journal, Vol. XLII. No. 6. 8vo. 1922.
Smithsonian Institution—Miscellaneous Collections, Vol. LXXII. Nos. 13–14; Vol. LXXIII. No. 1. 8vo. 1922.
South Africa, Union of—Journal of Department of Agriculture, Vol. IV. No. 5. 8vo. 1922.
Tôhoku Imperial University—Science Reports, Vol. X. No. 6. 8vo. 1922.
United States Bureau of Standards—Scientific Papers, Nos. 428–430. 8vo. 1922.
Technologic Papers, Nos. 207–208. 8vo. 1922.
Circulars, No. 119. 8vo. 1922.
United States Department of Agriculture—Journal of Agricultural Research, Vol. XXII. No. 9. 8vo. 1922.
United States Geological Survey—World Atlas of Commercial Geology. Part II. fol. 1921.
United States Patent Office—Official Gazette, Vol. CCXCVII. No. 4.—Vol. CCXCVIII. No. 3. 8vo. 1922.
Washington National Academy of Sciences—Proceedings, Vol. VIII. No. 5. 8vo. 1922.
Western Australia—Quarterly Statistical Abstract, No. 224. 8vo. 1922.
Yorkshire Philosophical Society—Annual Report, 1921. 8vo. 1922.
Zoological Society—Proceedings, 1922. Part I. 8vo.

GENERAL MONTHLY MEETING,

Monday, July 3, 1922.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. J.P. F.R.S., Treasurer and Vice-President, in the Chair.

Viscountess D'Arcy,
Miss Eva Fairfax,
Sir Thomas Fisher, K.B.E.
Mrs. Grove-Hills,
John Hetherington,
Albert John Lambert,
Walter Francis Roch,
Captain M. H. P. Riall Sankey, C.B. C.B.E.
Lieut.-Col. J. A. Stirling, D.S.O. M.C.
Clarence Tierney,

were elected Members.

The Chairman announced the decease of H.H. The Prince of Monaco, on June 26, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to place on their Records a recognition of their sense of the great loss the Institution and the Science of Oceanography have sustained by the lamented death of His Highness Albert the First, Sovereign Prince of Monaco; Grand Cross of the Legion of Honour; Foreign Associate of the Academy of Sciences, Paris; Doctor of Laws in the Universities of Edinburgh and Aberdeen; Honorary Fellow of the Royal Geographical Society; and one of the Honorary Members of Elevated Rank in the Royal Institution.

The Prince of Monaco, who was a generous and munificent benefactor to Oceanography, founded and endowed an Oceanographical Institute in Paris, and a magnificent Museum and Laboratory in Monaco. His numerous marine expeditions, the scientific exploration of the lakes of the Azores, and his Arctic voyages have yielded fruitful results. The researches are recorded from 1889 up to the present time in a series of monumental and elaborate monographs entitled "Résultats des Campagnes Scientifiques accomplies sur son Yacht par Albert 1er Prince Souverain de Monaco," and the "Bulletin de l'Institut Océanographique," copies of which have been presented by the Prince to the Library of the Royal Institution.

Besides the interest the Prince of Monaco took in Oceanography was the study of Human Archæology, and he founded the Institute of Prehistoric Archæology in Paris. His valuable discoveries in the Font de Gaume Cavern in the Dordogne and the Mentone Caves were published in 1875 and 1906–1911.

The Prince of Monaco delivered a Friday Evening Discourse on "The Progress of Oceanography" at the Royal Institution on May 27, 1904.

The Managers desire to express on behalf of the Members their sincere sympathy with His Highness's Family in their bereavement.

The Special Thanks of the Members were returned to Sir Robert Hadfield, Bart., J.P. D.Sc. F.R.S. M.R.I., for a Donation of £500, and to Sir David Salomons, Bart., D.L. J.P. M.R.I., for a Donation of £25 to the Fund for the Promotion of Experimental Research.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XVII. Part 3. 8vo. 1922.

Memoirs of the Department of Agriculture, Botanical Series, Vol. XI. No. 7. 8vo. 1922.

British Museum Trustees—English Schools of Illumination, Part IV. fol. 1922.

Catalogue of Greek Coins: Arabia, Mesopotamia, Persia. 8vo. 1922.

Catalogue of Engraved British Portraits, Vol. V. 8vo. 1922.

Accademia dei Lincei—Rendiconti: Classe di Scienze Morali, Serie Quinta, Vol. XXX. Nos. 4–12. 8vo. 1921–22.

Astronomical Society, Royal—Monthly Notices, Vol. LXXXII. No. 7. 8vo. 1922.

British Architects, Royal Institute of—Journal, Third Series, Vol. XXIX. No. 16. 4to. 1922.

British Astronomical Association—Journal, Vol. XXXII. No. 7. 8vo. 1922.
British Dental Association—Journal, Vol. XLIII. Nos. 12–13. 8vo. 1922.
Cambridge Observatory—Report, 1921–22. 8vo.
Chemical Industry, Society of—Journal, June 1922. 8vo.
Chemical Society—Journal and Proceedings, June 1922. 8vo.
Civil Engineers, Institution of—Proceedings, Vol. CCXII. 8vo. 1921.
Cornell University, Agricultural Experiment Station—Memoirs, Nos. 46, 49–52. 8vo. 1921–22.
Editors—Animals' Defender, July 1922. 8vo.
Chemical Abstracts, Jan.–May 1922. 8vo.
Chemical News, June 1922. 4to.
Chemist and Druggist, June 1922. 8vo.
Dyer and Calico Printer, June 1922. 4to.
Engineer, June 1922. fol.
Engineering, June 1922. fol.
Journal of Physical Chemistry, May 1922. 8vo.
Junior Mechanics, June 1922. 8vo.
Law Journal, June 1922. 8vo.
Le Petit Parisien, June 1922. fol.
Model Engineer, June 1922. 8vo.
Musical Times, June 1922. 8vo.
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Physical Review, May 1922. 8vo.
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Wireless World, June 1922. 8vo.
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Fleming, Prof. J. A., M.A. D.Sc. F.R.S. M.R.I. (the Author)—Michael Faraday and the Foundations of Electrical Engineering (Journ. Inst.E.E. Vol. LX. No. 308). 8vo. 1922.
Florence, Biblioteca Nazionale—Bollettino, Jan.–Feb. 1922. 8vo.
Florence, R. Accademia dei Georgofili—Atti Quinta Serie, Vol. XIX. Disp. 1. 8vo. 1922.
Franklin Institute—Journal, Vol. CXCIII. No. 6. 8vo. 1922.
Horological Institute—Horological Journal, July 1922. 8vo.
Illuminating Engineering Society—Illuminating Engineer, Vol. XV. No. 3. 8vo. 1922.
Imperial Institute—Bulletin, Vol. XX. No. 1. 8vo. 1922.
Iron and Steel Institute—Carnegie Scholarship Memoirs, Vol. XI. 8vo. 1922.
John Fritz Medal Board of Award—The John Fritz Medal Book, Second Edition. 8vo. 1922.
Johns Hopkins University—American Journal of Philology, Vol. XLIII. No. 2. 8vo. 1922.
Studies, Series XXXIX. Nos. 2–3. 8vo. 1921.
University Circulars, 1921, Nos. 2–6; 1922, No. 1. 8vo.
Life-Boat Institution—Annual Report, 1921. 8vo. 1922.
Linnean Society—Journal, Botany, Vol. XLVI. No. 305; Zoology, Vol. XXXV. No. 231. 8vo. 1922.
London County Council—Gazette, June 1922. 4to.
Madrid, Real Academia de Ciencias—Revista, Tome XIX. Nos. 1–6. 8vo. 1921.
Memorias, Ser. 2, Tome II. 8vo. 1921.
Discursos de S. M. El Rey D. Alfonso XIII. 8vo. 1922.
Anuario, 1922. 12mo.
Meteorological Office—Professional Notes, Nos. 27 & 29. 8vo. 1922.
Monaco, Institut Océanographique—Bulletin, Nos. 409–413. 8vo. 1922.
National Academy of Sciences, Washington—Proceedings, Vol. VIII. No. 6. 8vo. 1922.

New Zealand, The High Commissioner—Statistics of the Dominion, 1920. Vol. III. 4to. 1921.
Paris, Société Française de Physique—Journal de Physique et le Radium, Tome III. No. 5. 8vo. 1922.
Pharmaceutical Society of Great Britain—Journal, June 1922. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLVI. No. 7. 8vo. 1922.
Physical Society—Proceedings, Vol. XXXIV. Part 4. 8vo. 1922.
Rockefeller Institute for Medical Research—Studies, Vol. XL. 8vo. 1922.
Röntgen Society—Journal, Vol. XVIII. No. 72, July 1922. 8vo.
Royal Engineers' Institute—Journal, Vol. XXXVI. No. 1. 8vo. 1922.
Royal Society of Arts—Journal, June 1922. 8vo.
Royal Society of London—Proceedings, A, Vol. CI. No. 710; B, Vol. XCIII. No. 654. 8vo. 1922.
Salford, Borough of—Seventy-Third Report of Museums and Libraries Committee. 8vo. 1822.
Smithsonian Institution—Miscellaneous Collections, Vol. LXXII. No. 15. 8vo. 1922.
Stanford University—Publications: Biological Sciences, Vol. I. Nos. 3-4; Vol. II. No. 2; Mathematics, Vol. I. No. 1. 8vo. 1921.
Statistical Society, Royal—Journal, Vol. LXXXV. Part 3. 8vo. 1922.
Swiss Chemical Society—Helvetica Chemica Acta, Vol. V. Fasc. 4. 8vo. 1922.
Tôhoku Imperial University—Science Reports, 2nd Series (Geology), Vol. VI. No. 1. 4to. 1922.
United States Coast and Geodetic Survey—Magnetic Observations at Cheltenham and Vicques, 1917-1918. 4to. 1922.
United States Geological Survey—Geologic Atlas, No. 213. fol. 1921.
United States Patent Office—Official Gazette, Vol. CCXCVIII. No. 4—Vol. CCXCVIII. No. 5. 8vo. 1922.
Vienna Geologischen Gesellschaft—Mitteilungen, Band XIV. Heft 1. 8vo. 1921.
Yale University, Library—The Evolution of Modern Medicine. By Sir William Osler. (Silliman Lectures, 1913.) 8vo. 1921.
The Intestinal Flora. By L. F. Rettger and H. A. Cheplin. 8vo. 1921.

GENERAL MONTHLY MEETING,

Monday, November 6, 1922.

Sir James Crichton-Browne, M.D. LL.D. J.P. F.R.S., Treasurer and Vice-President, in the Chair.

Viscount Falmouth,
Michael Grabham, M.D. F.R.C.P.
N. Miesegaes,

were elected Members.

The Chairman announced the decease of Lord Scott Dickson, on August 4, and of Colonel E. H. Grove-Hills, on October 2, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record in their Minutes the deep sense of the great loss the Institution has sustained by the death of the Right Hon. Lord Scott Dickson, Lord Justice Clerk, Privy

Councillor, K.C. J.P. D.L., Fellow of the Royal Society of Edinburgh, Honorary Bencher of the Middle Temple, LL.D. of Glasgow and Aberdeen Universities, Member of Parliament for Glasgow, Bridgeton Division, 1900–06, and Central Division, 1909–10.

Lord Scott Dickson was admitted a Member of the Faculty of Advocates in 1877, and became Solicitor-General for Scotland in 1896, Lord Advocate in 1903, and Dean of Faculty in 1908.

A fitting recognition of his great eminence in the Legal World was shown by his appointment as Head of the Second Division of the Scottish Law Courts in 1915.

He was a distinguished and valued Member of the Judicial Committee of the Privy Council for hearing appeals from the Colonies; his last visit to London was in discharge of such duties, and his decease took place two days after the conclusion of the Session on his return to Scotland.

For twenty-seven years a Member of the Royal Institution, Lord Scott Dickson served on the Board of Managers, and his counsel and advice were always freely given for the welfare and progress of the Institution.

The Managers desire to express on behalf of the Members their sincere sympathy with Lady Scott Dickson and family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to place on record their sense of the great loss the Institution has sustained by the death of the Secretary and Vice-President, Colonel Edmond Herbert Grove-Hills, R.E. C.M.G. C.B.E. D.Sc. F.R.S. F.C.S., President of the Royal Astronomical Society, 1913–1915.

Colonel Grove-Hills was educated at Winchester and Woolwich, entered the Royal Engineers in 1884, was made a Captain in 1893, and in the same year became Assistant-Instructor in Chemistry and Photography at the School of Military Engineering, Chatham. He was a Member of the Observing Staff of the Solar Eclipse Expeditions to Africa (1893), Japan (1896), and India (1898). He was made Deputy Assistant Adjutant-General at Headquarters in 1899, and Major in 1901. In 1902 he acted as Secretary to the Commission on the delineation of the boundary between Chili and Argentina, and received the honour of C.M.G. in recognition of his services. While in the service of the Topographical Section of the General Staff at the War Office, Colonel Grove-Hills gave whole-hearted support to Sir David Gill's project for measuring a long arc of meridian from the South of Africa to Cairo. In 1903 he was allowed by the War Office to assist the Canadian Government in the survey of that country, and in 1905 he retired from the Royal Engineers. The Colonial Office appointed him to inspect and report upon the Geodetic Survey Departments in British East Africa, Uganda and Ceylon in 1907, and he continued survey work in Southern Nigeria in the following year. In the same year he was elected President of the Geographical Section of the British Association, when he delivered an address on "The Present and Future Work of the Geographer." In 1914 he was a member of the Observing Staff of the Solar Eclipse Expedition to Russia. On the outbreak of the War he rejoined his regiment, and was appointed Assistant Chief Engineer, Eastern Command. The same year he was made a Colonel, and in 1919 a Brigadier-General. For his services during the war he was honoured with the C.B.E. in 1919.

Colonel Grove-Hills was elected a Member of the Royal Institution in 1892, having been proposed by his distinguished grandfather, Mr. Justice Grove, and continued a Member for thirty years. He was elected to the Board of Visitors in 1907, to the Managerial Board in 1914, and made Secretary and Vice-President in 1915. In all these offices his zealous devotion to the Institution was shown by his untiring efforts in promoting its objects and interests. He delivered a Friday Evening Discourse in 1916, on "The Movements of the Earth's Pole."

On behalf of the Members, the Managers desire to express their sincere sympathy with Mrs. Grove-Hills and the family in their bereavement.

The Managers reported, That there was a vacancy in the Office of the Secretary, through the decease of Colonel E. H. Grove-Hills, C.M.G. F.R.S., and that at the next General Meeting on December 4 the vacancy will be filled in accordance with the Bye-Laws, Chapter IV., Article 2.

The Managers further reported, That the following Legacy had been received :—

W. A. M. Poynton . . £45.

Statement on disposal of books in the Library :—

The congestion of the Library and the want of space for new books had become a vital question. It was found that a large number of old editions amounting to about 965 volumes, could be disposed of, giving large additional space, without diminishing in any way the utility of the Library as a whole. The Resident Professor found that one of the Members, Dr. W. Rushton Parker, M.A. M.D., Gonville and Caius College, Cambridge, a distinguished graduate in Science and Mathematics, a retired practitioner, and author of original papers on medical subjects, who has a highly competent knowledge of Libraries, was willing to help the Institution by considering which books should be disposed of. Dr. Parker had already taken exceptional interest in the Library of the Royal Institution, having presented no less than forty-seven volumes, which constitutes him a generous benefactor. In the month of August, Dr. Parker kindly agreed to take the matter in hand, and had, at a great sacrifice of time and energy, devoted five to six weeks' continuous work in arranging for the disposal of surplus books.

Resolved, That the special thanks of the Managers be conveyed to Dr. W. Rushton Parker, M.A. M.D., Graduate of Gonville and Caius College, for the advice and special service he has given in the improvement of the Library, and for his handsome gift of forty-seven volumes, adding to the utility of the Library; and that this Resolution be reported to the Members at their General Meeting to be held this day.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Memoirs of the Department of Agriculture, Entomological Series, Vol. VIII. Nos. 7–8. 8vo. 1922.
Report of Board of Scientific Advice, 1920–21. 8vo. 1922.
Geological Survey: Records, Vol. LIII. Part 3; Vol. LIV. Part 1. 8vo. 1922. Palæontologia Indica, Vol. VI. No. 2. 4to. 1922.
Agricultural Journal, Vol. XVII. Part 4. 8vo. 1922.
Agricultural Research Institute, Pusa: Bulletin, No. 128. 8vo. 1922.
Report on Madras Government Museum, 1921–22. 4to.
Kodaikanal Observatory: Bulletin, No. LXX. 4to. 1922.

Lords of the Admiralty—Greenwich Observations, 1917. 4to. 1922.
Photoheliographic Results, 1917. 4to. 1922.
Cape Observatory Annals, Vol. XI. Parts 4–5. 4to. 1922.
Cape Astrographic Zones, Vol. V. 4to. 1922.
Aeronautical Society, Royal—Journal, July-Oct. 1922. 8vo.
Allegheny Observatory—Publications, Vol. VI. Nos. 4–5. 4to. 1922.
American Academy of Arts and Sciences—Proceedings, Vol. LVII. Nos. 3–10. 8vo. 1922.
American Geographical Society—Geographical Review, Oct. 1922. 8vo.
Antiquaries, Society of—Antiquaries Journal, Vol. II. Nos. 3–4. 8vo. 1922.
Asiatic Society, Royal—Journal, July-Oct. 1922. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. LXXXII. No. 8. 8vo. 1922.
Bangalore, Indian Institute of Science—Journal, Vol. V. Parts 1–3. 8vo. 1922.
Bankers, Institute of—Journal, Vol. XLIII. Parts 7–8. 8vo. 1922.
Belfast Philosophical Society—Proceedings, 1920–21. 8vo. 1922.
Belgium, Royal Academy of Sciences—Annuaire, 1922. 8vo.
Bulletin, 1921, No. 12; 1922, Nos. 1–7. 8vo. 1921–22.
Mémoires, 2e Série, Collection in 4to, Tome IV. Fasc. 7–8; Collection in 8vo, Tome VI. Fasc. 10–15; Tome VII. Fasc. 1. 1922.
Birkbeck College—Calendar, 1922–23. 8vo. 1922.
Boston Public Library—Bulletin, Vol. IV. Nos. 2–3. 8vo. 1922.
Botanic Society, Royal—Quarterly Summary, July 1922. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXIX. Nos. 17–20. 4to. 1922.
British Astronomical Association—Journal, Vol. XXXII. Nos. 8–9. 8vo. 1922.
British Dental Association—Journal, Vol. XLIII. Nos. 15–21. 8vo. 1922.
Cambridge Philosophical Society—Transactions, Vol. XXII. Nos. 23–25. 4to. 1922.
Canada, Department of Emigration—Atlas of Canada. fol. 1915.
Canada, Geological Survey—Bulletins, Nos. 34, 36. 8vo. 1922.
Memoirs, Nos. 126, 131. 8vo. 1922.
Summary Report, 1921, Parts A and D. 8vo. 1922.
Report on Structural Materials along the St. Lawrence River. 8vo. 1922.
Canadian Institute, Royal—Transactions, Vol. XIV. Part 1. 8vo. 1922.
Carnegie Institution, Mount Wilson Observatory—Contributions, Nos. 219–233. 8vo. 1922.
Communications to National Academy of Science, Nos. 77, 79, 82–83. 8vo. 1922.
Chemical Industry, Society of—Journal, July–Oct. 1922. 8vo.
Chemical Society—Journal and Proceedings, Aug.-Oct. 1922. 8vo.
Chemistry, Institute of—Journal and Proceedings, 1922, Parts 3–4. 8vo.
Chili, Instituto Central Meteorologico—Anuario, 1919. 8vo. 1922.
Civil Engineers, Institution of—List of Members, 1922. 8vo.
Cleveland Technical Institute—Bulletin, Vol. I. Nos. 9–11. 8vo. 1922.
Clodd, Edward, J.P. (the Author)—Occultism (R.I. Lectures, 1921). 8vo. 1922.
Colonial Institute, Royal—United Empire, July-Nov. 1922. 8vo.
Cornell University, Agricultural Experiment Station—Bulletin, Nos. 408–410; Memoirs, No. 54. 8vo. 1922.
East India Association—Journal, N.S., Vol. XIII. Nos. 3–4. 8vo. 1922.
Eastman Kodak Co.—Scientific Publications, Vol. IV. 1919–20. 8vo. 1922.
Editors—Animals' Defender, Aug.-Nov. 1922. 8vo.
British Engineers' Journal, July-Oct. 1922. 4to.
Chemical Abstracts, June-Oct. 1922. 8vo.
Chemical News, July-Oct. 1922. 4to.
Chemist and Druggist, July-Oct. 1922. 8vo.
Civil Engineering, Oct. 1922. 4to.
Dyer and Calico Printer, July-Oct. 1922. 4to.

Editors—contd.
Engineer, July–Oct. 1922. fol.
Engineering, July–Oct. 1922. fol.
General Electric Review, July–Oct. 1922. 8vo.
Journal of Physical Chemistry, June–Oct. 1922. 8vo.
Junior Mechanics, July–Oct. 1922. 8vo.
Law Journal, July–Oct. 1922. 8vo.
Model Engineer, July–Oct. 1922. 8vo.
Musical Times, July–Oct. 1922. 8vo.
Nation and Athenæum, July–Oct. 1922. 4to.
Nature, July–Oct. 1922. 4to.
New Church Magazine, July–Aug. 1922. 8vo.
Nuovo Cimento, May–June, 1922. 8vo.
Physical Review, July–Sept. 1922. 8vo.
Science Abstracts, May–Aug. 1922. 8vo.
Wireless World, July–Oct. 1922. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LX. Nos. 310-311. 8vo. 1922.
Faraday Society—Transactions, Vol. XVI. Part 1; Vol. XVII. Part 3. 8vo. 1922.
Field Museum of Natural History—Publication No. 210, Game Birds from Venezuela. 8vo. 1922.
Finland, Minister for Foreign Affairs—Trade and Industry of Finland. 4to. 1922.
Florence, Biblioteca Nazionale—Bolletino, March–Sept. 1922. 8vo.
Franklin Institute—Journal, Vol. CXCIV. Nos. 1–4. 8vo. 1922.
Year Book, 1922. 8vo.
Journal of Franklin Institute, First Series, Vol. I.–IV. 1825–27. 8vo.
Indexes. 1886–1896; 1896–1905; 1906–1915. 8vo.
Year Books, 1913–1921. 8vo.
Lecture Lists, 1914–1922. 8vo.
Gauthier-Villars et Cie. (the Publishers)—Discours et Mélanges. Par E. Picard. 8vo. 1922.
Geneva, Société de Physique—Mémoires, Vol. XXXIX. Fasc. 7. 4to. 1922.
Geographical Society, Royal—Journal, Vol. LX. Nos. 1–4. 8vo. 1922.
Geological Society of London—Journal, Vol. LXXVII. Parts 2–3. 8vo. 1922.
Abstracts of Proceedings, No. 1090. 8vo. 1922.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises, Serie III.; Physiologie, Tome VI. Liv. 4. 8vo. 1922.
Harvard College—Annual Report of the Astronomical Observatory, 1922. 8vo.
Harvard University—Contributions from the Jefferson Physical Laboratory Vol. XV. 8vo. 1921.
Horological Institute—Journal, Vol. LXIV. No. 768: Vol. LXV. Nos. 769–771. 8vo. 1922.
Horticultural Society, Royal—Journal, Vol. XLVII. Parts 2–3. 8vo. 1922.
Illuminating Engineering Society—Illuminating Engineer, Vol. XV. Nos. 4–6. 8vo. 1922.
Imperial College of Science—Calendar, 1922–23. 8vo. 1922.
Imperial Institute—Bulletin, Vol. XX. No. 2. 8vo. 1922.
Iron and Steel Institute—Journal, Vol. CV. 8vo. 1922.
List of Members, 1922. 8vo.
Johns Hopkins University—American Journal of Philology, Vol. XLIII. No. 3. 8vo. 1922.
Kyoto Imperial University—Memoirs: College of Science, Vol. V. Nos. 1–8; College of Engineering, Vol. II. No. 5. 8vo. 1922.
Life-Boat Institution—Journal, Vol. XXIV. No. 276, May 1922. 8vo.
Lincei, Reale Accademia Nazionale—Atti: Rendiconti: Classe di Scienze Fisiche, Vol. XXXI. 1° Semestre, Nos. 1–12; 2° Semestre, Nos. 1–2. 8vo. 1922.
Classe di Filologie, Vol. XXXI. Fasc. 1–4. 8vo. 1920.

Linnean Society—Journal, Botany, Vol. XLVI. Nos. 306–7; Zoology, Vol. XXXV. No. 232. 8vo. 1922.
Liverpool University—Calendar, 1922–3. 8vo. 1922.
Lockyer, W. J. S., M.A. Ph.D. (*the Author*)—Relationship between Solar Prominences and the Corona; and Annual Report on Norman Lockyer Observatory, 1922. 8vo. 1922.
London County Council—Gazette, July–Oct. 1922. 4to.
London Society—Journal, July–Oct. 1922. 8vo.
London University—Gazette, July–Aug. 1922. 4to.
Mechanical Engineers, Institution of—Proceedings, 1922, Vol. I. 8vo.
List of Members, 1922. 8vo.
Meteorological Office, Air Ministry—Professional Papers, Nos. 28–30. 8vo. 1922.
Geophysical Memoirs, No. 19. 4to. 1922.
Wireless Weather Manual. 8vo. 1922.
Report of Eleventh Meeting of International Meteorological Committee, 1921. 8vo. 1922.
British Rainfall, 1919. 8vo. 1920.
Meteorological Society, Royal—Journal, Vol. XLVIII. Nos. 203–204. 8vo. 1922.
Metropolitan Asylums Board—Annual Report, 1921–22. 8vo. 1922.
Mexico, Sociedad Cientifica "Antonio Alzate"—Memorias y Revista, Tome XL. Nos. 2–6. 8vo. 1922.
Microscopical Society, Royal—Journal, 1922, Parts 2–3. 8vo.
Montpellier, Académie des Sciences—Bulletin, July 1921–May 1922. 8vo.
National Physical Laboratory—Report, 1921. 8vo. 1922.
Collected Researches, Vol. XVI. 4to. 1921.
New York, Society for Experimental Biology—Proceedings, Vol. XIX. Nos. 7–8. 8vo. 1922.
New Zealand, High Commissioner for—Census, 1921, Part 1 and Appendices. 4to. 1921.
Official Year Book, 1921–22. 8vo.
Statistics, 1920, Vol. IV. 4to. 1922.
Onnes, Dr. H. Kamerlingh, Hon.M.R.I.—Communications from the Physical Laboratory, University of Leiden, No. 157. 8vo. 1922.
Paris, Académie des Sciences—Comptes Rendus, Tomes 167–169. 4to. 1918–19.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, May–June, 1922. 8vo.
Paris, Société Française de Physique—Journal de Physique et le Radium, Tome III. Nos. 6–9. 8vo. 1922.
Parker, W. Rushton, M.D. M.A. M.R.I.—The Building of the British Isles. By A. J. Jukes-Browne. 4th Edition. 8vo. 1922.
Stanford's Geological Atlas of Great Britain and Ireland. Edited by H. B. Woodward. With photographic supplement. 2 vol. 8vo. 1913–14.
The World's Minerals. By L. J. Spencer. 8vo. 1911.
Life of Hannibal. By Thomas Arnold. 8vo. 1903.
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Post Office Electrical Engineers—Journal, Vol. XV. Parts 2–3. 8vo. 1922.
Papers, No. 85. 8vo. 1921.
Rockefeller Institute for Medical Research—Studies, Vol. XLI. 8vo. 1922.
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Calendar, 1922. 8vo.
Royal Dublin Society—Scientific Proceedings, N.S., Vol. XVI. Nos. 35–39; Vol. XVII. Nos. 1–10. 8vo. 1922.
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Royal Society of London—Proceedings: A, Vol. CI. Nos. 711–713; Vol. CII. No. 714; B, Vol. XCIV. Nos. 655–657. 8vo. 1922.
Philosophical Transactions, A, Vol. CCIII. Nos. 605–609. 4to. 1922.
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Bulletin, 1922, Nos. 1–2; Science Bulletin, Nos. 22–25. 8vo. 1922.
Soward, Sir Alfred W., M.R.I.—De Alchemia. By Geber. 12mo. 1598.
Pretiosa Margarita Novella de Thesauro, ac pretiosissimo Philosophorum Lapide. 12mo. 1546.
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Sweden, Royal Academy of Science—Arkiv: Botanik, Band XVII., Band XVIII. Heft 1; Kemi, Band VIII. Heft 3–4; Matematik, Band XVI. Heft 3–4, Band XVI. Heft 1–2; Zoologi, Band XIV. Heft 3–4, Band XV. Heft 1. 8vo. 1922.
Årsbok, 1922. 8vo.
Meddelanden, Band IV. Heft 2. 8vo. 1922.
Les Prix Nobel, 1919–20. 8vo. 1922.
Swiss Chemical Society—Helvetica Chemica Acta, Vol. V. Fasc. 5. 8vo. 1922.
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Second Series, Geology, Vol. VII. No. 1. 4to. 1922.
Mathematical Journal, Vol. XXI. Nos. 1–2. 8vo. 1922.
Technology Reports, Vol. II. No. 4. 8vo. 1922.
Tokyo, Iwata Institute of Plant Biochemistry—Acta Phytochimica, Vol. I. No. 1. 8vo. 1922.
United States Bureau of Standards—Circulars, Nos. 120–122. 8vo. 1922.
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Technologic Papers, Nos. 209–215. 8vo. 1922.
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United States Patent Office—Official Gazette, Vol. CCXCIX. No. 2—Vol. CCCIII. No. 3. 8vo. 1922.
Washington, National Academy of Sciences—Proceedings, Vol. VIII. Nos. 7–10. 8vo. 1922.
Upsala, University of—Bulletin Mensuel de l'Observatoire Météorologique, Vol. LIII. 4to. 1922.
Washington, National Research Council—Sixth Annual Report. 8vo. 1922.
Western Australia—Quarterly Statistical Abstract, Nos. 225–226, 1922. 8vo.
Yale University—Respiration. By J. S. Haldane. (Silliman Lectures.) 8vo. 1922.
Zoological Society—Proceedings, 1922, Part 2. 8vo. 1922.
Index, 1911–1920. 8vo. 1922.

GENERAL MONTHLY MEETING,

Monday, December 4, 1922.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. J.P. F.R.S., Treasurer and Vice-President, in the Chair.

In accordance with the Bye Laws, Chapter IV. Article 2,

Sir Arthur Keith, M.D. LL.D. F.R.S. F.R.C.S.

was elected Secretary in succession to the late Colonel E. H. Grove-Hills, C.M.G. C.B.E. D.Sc. F.R.S.

Vilhelm F. K. Bjerknes (Bergen),
Paul Ehrenfest (Leiden),
Martin Knudsen (Copenhagen),
Irving Langmuir (Schenectady),
Georges Urbain (Paris),

were elected Honorary Members of the Royal Institution.

Miss Nora Forman,
Frederick Hyde,
John Baptist Kramer,
Alfonso Marconi,
John Moir Stirling Marriner.
R. B. Owens, D.S.O. D.Sc.
Miss Maude Tuson Thomas,

were elected Members.

The following Lecture Arrangements Before Easter 1923 were announced :—

HERBERT HALL TURNER, D.Sc. D.C.L. F.R.S., Savilian Prof. of Astronomy, Oxford. Ninety-seventh Course. Six Lectures on SIX STEPS UP THE LADDER TO THE STARS: 1. THE DISTANCE OF THE STARS; 2. THE DISCOVERY OF THE PLANET NEPTUNE; 3. PHOTOGRAPHING THE STARS; 4. THE SPECTROSCOPE AND ITS REVELATIONS; 5. TWO GREAT STREAMS OF STARS; 6. THE SIZE OF A STAR. On Dec. 28, 30, 1922; Jan. 2, 4, 6, 9, 1923.

F. G. DONNAN, C.B.E. D.Sc. F.R.S. M.R.I., Prof. of Inorganic and Physical Chemistry, University of London. Two Lectures on SEMI-PERMEABLE MEMBRANES AND COLLOID CHEMISTRY: 1. THE THEORY OF IONIC EQUILIBRIA AND SEMI-PERMEABLE MEMBRANES; 2. RELATION TO PROBLEMS OF COLLOID CHEMISTRY AND BIOLOGY. On *Tuesdays*, Jan. 16, 23.

R. D. Oldham, F.R.S. Two Lectures on The Character and Cause of Earthquakes. On *Tuesdays*, Jan. 30, Feb. 6.

A. C. Pearson, Litt.D., Regius Prof. of Greek, University of Cambridge. Two Lectures on Greek Civilization and To-day: 1. The Beginnings of Science; 2. Progress in the Arts. On *Tuesday*, Feb. 13, *Wednesday*, Feb. 21.

Sir Arthur E. Shipley, G.B.E. Sc.D. F.R.S., Master of Christ's College, Cambridge. Two Lectures on Life and its Rhythms: 1. Life and its Attributes; 2. Rhythm in Living Organism. On *Tuesdays*, Feb. 27, March 6.

Charles G. Seligman, M.D. F.R.S., Prof. of Ethnology, Univ. of London. Two Lectures on Rainmakers and Divine Kings of the Nile Valley. On *Tuesdays*, March 13, 20.

The Hon. John W. Fortescue, C.V.O. LL.D., Librarian at Windsor Castle. Two Lectures on 1. The British Soldier and the Regimental Officer at the Close of the Napoleonic War; 2. The Campaigns of the British Army, 1815–1838. On *Thursdays*, Jan. 18, 25.

I. M. Heilbron, D.S.O. D.Sc. F.C.S., Heath-Harrison Prof. of Organic Chemistry, University of Liverpool. Two Lectures on The Photosynthesis of Plant Products. On *Thursdays*, Feb. 1, 8.

B. Melvill Jones, M.A., Francis Mond Prof. of Aeronautical Engineering, University of Cambridge. Two Lectures on Recent Experiments in Aerial Surveying. On *Thursdays*, Feb. 15, 22.

Theodore Stevens, B.Met. Min.Eng. M.Inst.C.E. M.I.E.E. Two Lectures on Water Power of the Empire. On *Thursdays*, March 1, 8.

Lieut.-Colonel E. F. Strange, C.B.E., Keeper of Woodwork, Victoria and Albert Museum. Two Lectures on Japanese and Chinese Lacquer. On *Thursdays*, March 15, 22.

Sir Walford Davies, Mus.Doc. LL.D., Director of Music in the University of Wales. Two Lectures (with Illustrations by Members of the Temple Choir) on Speech Rhythm in Vocal Music. On *Saturdays*, Jan. 20, 27.

J. C. Squire, M.A.(Cambridge), Editor "London Mercury." Two Lectures on Subject in Poetry (with special reference to contemporary practice). On *Saturdays*, Feb. 3, 10.

Sir Ernest Rutherford, LL.D. D.Sc. F.R.S. M.R.I., Prof. of Natural Philosophy, and Cavendish Prof. of Experimental Physics, University of Cambridge. Six Lectures on Atomic Projectiles and their Properties. On *Saturdays*, Feb. 17, 24, March, 3, 10, 17, 24.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Secretary of State for India—Agricultural Journal, Vol. XVII. Part 5, 8vo. 1922.
Geological Survey: Records, Vol. LIV. Part 2. 8vo. 1922.

Accademia dei Lincei—Rendiconti: Classe di Scienze Fisiche, Serie Quinta, Vol. XXXI. Nos. 3–6. 1922.

Aeronautical Society, Royal—Journal, Nov. 1922. 8vo.

American Academy of Arts and Sciences—Proceedings, Vol. LVII. Nos. 11–15. 8vo. 1922.

American Philosophical Society—Proceedings, Vol. LXI. Nos. 1–2. 8vo. 1922.
List of Members, 1922. 8vo.

Antiquaries, Society of—Archæologia, Vol. LXXI. 1920–21. 4to. 1921.

Astronomical Society, Royal—Monthly Notices, Vol. LXXXII. No. 9. 8vo. 1922.

Bankers, Institute of—Journal, Vol. XLIII. Part 9. 8vo. 1922.

Batavia, Royal Magnetical Observatory—Observations, Vol. XL. 1917. 4to. 1922.
Botanic Society, Royal—Quarterly Summary, Oct. 1922. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XXX. Nos. 1–2. 4to. 1922.
British Astronomical Association—Journal, Vol. XXXIII. No. 1. 8vo. 1922.
Handbook for 1923. 8vo. 1922.
List of Members, 1922. 8vo.
British Chemical Manufacturers, Association of—Shall the State throw away the Keys? (with Foreword by Sir W. Pope). 8vo. 1922.
British Dental Association—Journal, Vol. XLIII. Nos. 22–23. 8vo. 1922.
Cambridge Philosophical Society—Proceedings, Vol. XXI. Part 3. 8vo. 1922.
Canada, Department of Mines—Talc in Canada. 8vo. 1922.
Chemical Industry, Society of—Journal, Nov. 1922. 8vo.
Chemical Society—Journal and Proceedings, Nov. 1922. 8vo.
Chemistry, Institute of—Journal and Proceedings, 1922, Part 5. 8vo.
Cleveland Technical Institute—Bulletin, Vol. II. No. 1. 8vo. 1922.
Colonial Institute, Royal—United Empire, Vol. XIII. No. 12. 8vo. 1922.
Editors—Animals' Defender, Dec. 1922. 8vo.
British Engineers' Journal, Nov. 1922. 8vo.
Chemical Abstracts, Nov. 1922. 8vo.
Chemical News, Nov. 1922. 4to.
Chemist and Druggist, Nov. 1922. 8vo.
Civil Engineering, Dec. 1922. 4to.
Dyer and Calico Printer, Nov. 1922. 4to.
Engineer, Nov. 1922. fol.
Engineering, Nov. 1922. fol.
General Electric Review, Nov. 1922. 8vo.
Journal of Physical Chemistry, Nov. 1922. 8vo.
Junior Mechanics, Nov. 1922. 8vo.
Law Journal, Nov. 1922. 8vo.
Model Engineer, Nov. 1922. 8vo.
Musical Times, Nov. 1922. 8vo.
Nation and Athenæum, Nov. 1922. 4to.
Nature, Nov. 1922. 4to.
New Church Magazine, Nov.–Dec. 1922. 8vo.
Nuovo Cimento, July–Sept. 1922. 8vo.
Physical Review, Oct.–Nov. 1922. 8vo.
Science Abstracts, Sept.–Oct. 1922. 8vo.
Terrestrial Magnetism, Vol. XXVII. No. 3, Sept. 1922. 8vo.
Wireless World, Nov. 1922. 8vo.
Electrical Engineers, Institution of—Journal, Vol. LX. No. 312, Aug. 1922. 8vo.
Faraday Society—Transactions, Vol. XVIII. Part 1. 8vo. 1922.
Florence, Biblioteca Nazionale—Bollettino, Oct.–Nov. 1922. 8vo.
Franklin Institute—Journal, Vol. CXCIV. No. 5. 8vo. 1922.
Geographical Society, Royal—Geographical Journal, Vol. LX. No. 5, 1922. 8vo.
Geological Society—Abstracts of Proceedings, Nos. 1091–1092. 8vo. 1922.
Horological Institute—Horological Journal, Dec. 1922. 8vo.
Imperial Institute—Bulletin, Vol. XX. No. 3. 8vo. 1922.
Johnston-Lavis, Madame F.—Bibliography of the Volcanoes of Southern Italy. By H. J. Johnston-Lavis. 2nd edition. 8vo. 1918.
Leiden University, Physical Laboratory Executive Committee—Het Natuurkundig Laboratorium des Rijksuniversiteit te Leiden in de Jaren, 1904–1922. 8vo. 1922.
Linnean Society—Proceedings, 134th Session. 8vo. 1922.
List of Fellows, 1922–23. 8vo. 1922.

Lisboa, Instituto de Medicina Legal—Archivo de Medicina Legal, Vol. I. Nos. 1-2. 8vo. 1922.
London County Council—Gazette, Nov. 1922. 4to.
London Society—Journal, Nov. 1922. 8vo.
London University—Gazette, Nov. 1922. 4to.
Meteorological Society, Royal—Bibliography of Meteorological Literature, No. 2. 8vo. 1922.
Mexico, Sociedad Cientifica, "Antonio Alzate"—Memorias, Tome XLI. No. 1. 8vo. 1922.
Monaco, Institut Océanographiques—Prince Albert I., Campagnes Scientifiques, Fascs. 61-63. 4to. 1922
Miscellaneous Papers, 4to and 8vo, 1886-1907.
Bulletin, Nos. 414-418, 420. 8vo. 1922.
Mond, Robert, J.P. M.A. M.R.I.—Bibliographical History of Electricity and Magnetism. By P. F. Mottelay. 8vo. 1922.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin, Aug.-Oct. 1922. 8vo.
Paris, Société Française de Physique—Journal de Physique et le Radium, Tome III. No. 10. 8vo. 1922.
Parker, W. Rushton, M.D. M.R.I.—The Glastonbury Lake Village. By A. Bulleid and N. St. G. Gray. 2 vols. 4to. 1911-17.
The Age of Mammals By H. F. Osborn. 8vo. 1921.
Peru, Consul-General—The President's Message to Congress. 4to. 1922.
Pharmaceutical Society of Great Britain—Journal, Nov. 1922. 8vo.
Photographic Society, Royal—Journal, N.S., Vol. XLVI. No. 12. 8vo. 1922.
Roumanian Academy—Bulletin, 1920-21, Nos. 7-10: 1922-23, Nos. 1-2. 8vo. 1922.
Royal Engineers' Institute—Journal, Vol. XXXVI. No. 6. 8vo. 1922.
Royal Society of Arts—Journal, Nov. 1922. 8vo.
Royal Society of London—Proceedings, A, Vol. CII. No. 115; B, Vol. XCIV. No. 658. 8vo. 1922.
Smithsonian Institution—Miscellaneous Collections, Vol. LXXIV. Nos. 2-4. 8vo. 1922.
Annals of the Astrophysical Observatory, Vol. IV. 4to. 1922.
South Africa, Union of—Journal of Department of Agriculture, Nov. 1922. 8vo.
Tôhoku Imperial University—Science Reports, 3rd Series, Vol. I. No. 2. 8vo. 1922.
United States Bureau of Standards—Scientific Papers, Nos. 440, 443. 8vo. 1922.
Circulars, Nos. 123-132. 8vo. 1922.
United States Patent Office—Official Gazette, Vol. CCCIII. No. 4—Vol. CCCIV. No. 3. 8vo. 1922.
Yerkes Observatory—Publications, Vol. III. Parts 1-3; Vol. IV. Parts 1-3. 4to. 1903-20.
Yorkshire Archæological Society—Journal, Vol. XXVI. Part 4 (No. 104). 8vo. 1922.
Year's Work in Archæology, 1921. 8vo. 1922.
Zoological Society—Proceedings, 1922, Part III. 8vo.
List of Fellows, 1922. 8vo.

WEEKLY EVENING MEETING,

Friday, May 12, 1922.

JOHN MITCHELL BRUCE, Esq., C.V.O. M.A. M.D. LL.D. F.R.C.P., Manager and Vice-President, in the Chair.

H. H. DALE, C.B.E. M.D. F.R.S., Head of Department of Biochemistry and Pharmacology under Medical Research Council.

The Search for Specific Remedies.

[ABSTRACT.]

THE idea of discovering for each disease a specific remedy is not a new one, and some of those discovered in the pre-scientific period are still the best available for certain infections. The study of immunity, and of the remedies produced by the natural reaction of the body, for a time diverted interest from the artificial medicines in which mankind had formerly trusted, and brought them into discredit. When the limitations of immunological therapeutics began to appear, the search for artificial remedies, for infections to which little natural immunity is developed, began on new lines and with new aims, which the study of Nature's methods had indicated. The search now to be undertaken, with the newly gained resources of synthetic chemistry, was for substances which should resemble the natural antibodies in being specifically harmful for the parasite and harmless for the patient —in Ehrlich's phraseology, maximally parasitotropic and minimally organotropic.

The problem might well have appeared hopeless. What little is known of the chemistry of the infective micro-organisms suggests that it is very similar to that of our own body cells, and the problem was to find a chemical substance adapted to combine with the one and not with the other.

Ehrlich started his search by investigating a number of synthetic dyes, his use of which as microchemical reagents has shown their differential affinities for different types of cell. He had at disposal the technique of transmitting infections with trypanosomes developed by Laveran and Mesnil. A dye of the benzidine series was found which cured mice infected with the trypanosome of "Mal de Caderas," and to this the name "trypan-red" was given. It had little curative action, however, on infections with the trypanosomes of other diseases, or on the infection of other species with that of "Mal de Caderas." Nor were the trypanosomes of "Mal de Caderas" obviously injured

by the trypan-red *in vitro*. It seems obvious that the organism of the mouse did not play a purely passive and indifferent part in the curative process. A related and more effective dye, trypan-blue, was introduced by Mesnil and Nicolle, and has found practical application in the treatment of piroplasmosis.

Though substances of this series were originally investigated on account of their properties as dyes, it has become evident that their dyeing action had no essential connection with their curative effects. The latest and most important development of this line of enquiry has been the introduction of the substance known, as yet, only by a number, as "Bayer 205." This is known to have resulted from investigations suggested by the action of trypan-blue, and of a further member of the same series, afridol-violet. But "205" is itself a colourless substance, extremely soluble in water, and having none of the properties of a dye. Its composition is still a secret, but it has already proved to have remarkable curative properties, not only on experimental trypanosome infections in small animals, but on the naturally acquired trypanosomiasis of man, the deadly and previously incurable African sleeping-sickness.

The properties and mode of action of "205" are very different from those contemplated at the initiation of the search along these lines. There is no evidence that it has a specific affinity for the parasites. When treated with it directly they are not visibly harmed, though they lose the power of infection. Most remarkable is the persistence of its effect in the body, the animal which has received an injection being resistant to infection with trypanosomes for weeks or even months.

Another line of investigation which has been fruitful in result is the search for curative compounds containing arsenic, antimony and bismuth. Thomas and Breinl found that the organic compound of arsenic, known as atoxyl, was much more effective in the treatment of experimental trypanosomiasis than arsenic in organic form. The discovery of the true structure of atoxyl by Ehrlich and Bertheim provided the starting point for the production of a long series of derivatives, with the ultimate discovery, in "salvarsan," of a substance which has found application of immense importance in the treatment of spirochætal infections, including syphilis. Here again, with salvarsan and other arsenical derivatives, we find that the parasite is not killed by the direct application of the remedy outside the body. There is, however, a clue to the nature of their action afforded by the observation that the arsenious oxides, formed by reduction of substances like atoxyl, or partial oxidation of those like salvarsan, are immensely more toxic to the parasites, and incidentally to the patient, than the parent substances. It is probable that the slow liberation in the body of these oxides, which are too immediately toxic for direct administration, gives the compounds of this series their curative power.

In the case of antimony and bismuth the production of complex organic derivatives has been neither successful nor necessary. Relatively simple salts, and even the free metal in a fine state of division, have been found to possess valuable specific curative properties against certain forms of infection. The most familiar of all antimony compounds, tartar emetic, has proved to be a specific not only for a protozoal infection, such as kala-azar, but for infection by the small trematode worm Bilharzia; but the mode of its action is still a mystery.

In the action of emetine, the chief alkaloid of ipecacuanha, on amœbic dysentery, we have another example of a curative effect which cannot be explained by a direct killing of the parasites. Other alkaloids can be found, and even derivatives of emetine itself, which are more harmful to the amœbæ outside the body, much less poisonous to man than emetine, but devoid of curative action on human amœbic dysentery. Emetine, moreover, is powerless to check the course of an artificial infection produced in a kitten with amœbæ from a case of human dysentery, which readily yields to emetine treatment. Again we are forced to the conclusion that the co-operation of the body of the host is an essential factor in the curative action.

The discovery of the nature of this co-operation, by the body of the infected animal, in the action of the many artificial specific remedies now available, would appear to be an essential step towards a more orderly advance in this field of investigation.

[H. H. D.]

WEEKLY EVENING MEETING,

Friday, April 7, 1922.

SIR JAMES REID, BART., G.C.V.O. K.C.B. M.D. LL.D., Vice-President, in the Chair.

SIR ERNEST RUTHERFORD, LL.D. D.Sc. F.R.S. M.R.I.

Evolution of the Elements.

[ABSTRACT DEFERRED.]

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